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# THE SCIENTIFIC MONTHLY

## MAY, 1921

## THE HISTORY OF MATHEMATICS*

By Professor ERNEST W. BROWN

YALE UNIVERSITY

THE earliest dawn of science is without doubt not different from that of intelligence. But the civilized man of to-day, far removed as he is from the lowest of existing human races, is probably as far again from the being whom one would not differentiate from the animals as far as mental powers are concerned. What this difference is, neither ethnologist nor psychologist can yet tell. Perhaps the nearest approach to a definition, at least from the point of view of this article, is contained in the distinction between unconscious and conscious observation. We are familiar with both sides even in ourselves: records can be impressed on the brain and remain there apparently dormant until some stimulus brings them to fruition, and again, record and stimulus can appear together so that a train of thought is immediately started.

The faculty of conscious observation is a fundamental requirement of a scientific training, and no development can take place until it has been acquired to some extent. Although this is only the first step, it was probably a long one in the history of the race, just as it is relatively long in the lives of the majority of individuals. Reasoning concerning the observation follows, but not much success can be attained until a considerable number of observations have been accumulated. We may indeed put two and two together to make four, but experience shows that with phenomena the answer is more often wrong than right: we need much more information in order to get a correct answer.

We expect, then, that the earlier stage of a science will be one not so much of discovery as of conscious observation of phenomena which are apparent as soon as attention is called to them. The habit once acquired, the search for the less obvious facts of nature begins, and

*A lecture delivered at Yale University, February 26, 1920, the first of a series on the History of Science under the auspices of the Gamma Alpha Graduate Scientific Fraternity.

in the search many unexpected secrets are found. Once a body of facts has been accumulated, correlation follows. The attempt is made to find something common to them all and it is at this stage that science, in the modern sense, may perhaps be said to have its birth. But this is only a beginning. The mind that can grasp correlation can soon proceed to go further and try to find a formula which will not only be a common property, but which will completely embrace the facts; that is, in modern parlance, a law which groups all the phenomena under one head.

The formula or law once discovered, consequences other than those known are sought, and the process of scientific discovery begins. Realms which could never have been opened up by observation alone are revealed to the mind which has the ability to predict results as consequences of the law, and thence is found the means by which the truth of the law is tested. If the further consequences are shown to be in agreement with what may be observed, the evidence is favorable. If the contrary, the law must be abandoned or changed so as to embrace the newly discovered phenomena. The process of trial and error, or of hypothesis and test, is a recurring one which embraces a large proportion of the scientific work of to-day.

Scientific development has two main aspects. One is the framing of laws in order to discover new phenomena and develop the subject forward so as to open out new roads into the vast forest of the secrets of nature. The other is the turning backward in order to discover the foundations on which the science rests. Just as no teacher would think it wise to start the young pupil in chemistry or physics by introducing him at the outset to the fundamental unit of matter or energy as it is known at the time, but will rather start in the middle of the subject with facts which are within the comprehension of his mind and the experience of his observation, science itself has been and must necessarily be developed in the same manner. We proceed down to the foundations as well as up to the phenomena.

This two-fold aspect of scientific research has had revolutionary results in the experience of the last half century. It has fundamentally changed the ideas of those who study the so-called laboratory subjects in which observation with artificially constructed materials goes hand in hand with the framing of laws and hypotheses, but it has changed the study of mathematics in an even more fundamental manner. In the past, geometry and arithmetic were suggested by observation and practical needs and the development of both with symbolic representation proceeded on lines which were dictated by the problems which arose. The methods of discovery in working forward were not essentially different from those of an observational science, except perhaps that the testing of a new law was unnecessary on account of the

rules of reasoning which accompanied them and became embodied into a system of logic. It was, however, in proceeding backward to discover the foundations that the whole aspect of the subject changed. While many of the hypotheses were suggested by observation and were known by numerous tests to be applicable to the discussion of natural phenomena, it became evident that the actual hypotheses used were independent of the phenomena. The laws which are at the basis of geometrical reasoning are not necessarily natural laws: they can equally well be regarded as mere productions of the brain in the same sense as we might imagine a race of intelligent beings on another planet free from some of our limitations or restricted by limitations from which we are free. It is seen to be the same with the rules of symbolic reasoning which have gradually grown up. A geometry without Euclid's axiom of parallels has been constructed perfectly consistent in all its parts. This is built up of a set of axioms which constitute its foundation together with a code of reasoning by which we develop the consequences of the axioms. The same is true of geometries which involve space in more than three dimensions. It is somewhat easier to imagine symbolic developments which have their foundations different from those of our school and college algebras because there is no obvious connection between these rules and the phenomena of nature.

It may be asked what limitation is there in the development of mathematical theories if any set of axioms may be laid down. Theoretically there is none, except that if we retain our code of reasoning about them such axioms must not be inconsistent with one another. A certain sense of the fitness of things restrains mathematicians from a wild overturning of the law and order which have been established in the development of mathematics, just as it restrains democracies from trying experiments in government which overturn too much the existing order of affairs. Changes proceed in mathematics just as in politics by evolution rather than by revolution. The slowly built up structure of the past is not to be lightly overturned for the sake of novelty.

The developments traced above apply mainly to the subject of mathematics apart from its applications to the solution of problems presented by nature. Applied mathematics is a method of reasoning through symbols by which we can discover the consequences of the assumed laws of nature. The symbolism which we adopt and the rules we lay down with which to reason are immaterial, provided they are convenient for the objects we have in view. One feature must not be forgotten. We can never deduce the existence of phenomena through mathematical processes which were not implicitly contained in the laws of nature expressed at the outset in symbols. One cannot take out of the mathematical mill any product which was not present in the raw

material fed into it. It is the purpose of the mill to work up the raw material and the better the machinery the more finished and more varied will be the product.

To write a connected story of mathematical development within the limits of a brief article on a consistent plan without making it a mere catalogue of names and results is a difficult, perhaps impossible, task and no attempt is made here to accomplish it. If we try to lay stress on the workers rather than on what they achieved, in one period we encounter schools which developed particular subjects, in another, outstanding figures with or without influence on contemporary development and in still another numerous investigators who contributed in varying degrees to the advances made in their age. On the other hand if the development of ideas be the basis, parallel developments followed independently by different schools sometimes occur, at another time general methods of treatment seem to pervade and again we may find some fundamental advance made, the effect of which is not felt for many years. Consequently the plan which seems to fit best any particular period has been adopted for that period. Another difficulty consists in assigning the relative values to either men or ideas, about which probably no two persons will agree. There is, however, one stumbling block which is peculiar to mathematics. The very names themselves of many important branches of pure mathematics convey no meaning to the majority of scientific readers who are not trained mathematicians and to whom alone this article is intended to appeal. An attempt at brief definition has sometimes been made, but it can at best only give a partial view of the subject even with concrete illustrations of its significations. In the general outline, the historical development has been followed, but the methods of carrying it forward have varied with different periods. When a choice of names mentioned has to be made, a rough guide has been furnished in the earlier periods by selecting those who have taken the step forward which has rendered the subject capable of expansion or application by others as judged in the light of present knowledge. In the nineteenth and present centuries to carry out this method has proved to be beyond the ability of the writer; consequently, in most cases personal mention has only been incidental and the names of many of those who have done great service are missing. Fortunately, the history of mathematics has received much attention in articles and separate volumes and to them the reader who is interested in obtaining fuller information is referred.

The earliest traces in the form of written records have come to us from the Babylonians, mainly in the form of clay tablets which appear

to have been made at least 2000 years B. C. They show a knowledge of numbers which indicates that their civilization must have been far removed from the low stages in which many native tribes exist at the present day. Simple counting with the fingers of the two hands can be considered as the first stage, but beyond ten some new system must be devised. It appears that the Babylonians had learned the method of position, that is, that the first figure to the right shall represent the units, the next figure the tens and so on. They had even constructed numbers with 60 as the base of the system instead of ten. They could write numbers exceeding a million, one tablet giving a table of squares, and they also used fractions. Their geometry, however, was only in an elementary stage. But in astronomy they seem to have passed beyond the first stage of observation and to have been able to classify the results, for they possessed a knowledge of the Saros or period of 18, 2/3 years in which the eclipses of the sun and moon recur; this must have involved a long period of observation and record as well as the ability to classify the results and it may perhaps be regarded as the earliest recorded scientific deduction from observation.

Concurrently with this civilization was one of perhaps equal development in Egypt. The Ahmer Papyrus, which is usually dated some 2000 years B. C., shows that the Egyptians had not only already constructed an arithmetic but had started the solution of what we now call equations of the first degree with one unknown. There was no general method for solving the problems and no symbolism for the unknown, although symbols for addition, subtraction and equality occur. In geometry they were also somewhat in advance of the Babylonians, apparently on account of practical needs in land surveying. It is generally agreed that the pyramids show evidence of astronomical observation in their orientation with respect to the stars and they certainly show evidence of a knowledge of geometrical form. As these monuments appear to go back some 4000 years B. C., we have evidence of some considerable development much further back even than the times indicated by the Babylonian tablets or the Ahmer Papyrus.

But the first real evidence of the scientific spirit comes from Greece. While they probably inherited some of the accumulated knowledge of both Babylonia and Egypt, they transformed much of the raw material thus acquired into a finished product which has survived to our own times. Much the most remarkable of all that we inherit from them in science, is the change which they effected in the study of geometry. The love and knowledge of form which is so strikingly exhibited in their buildings and statuary was also applied to geometrical figures. Development of logic and a real desire to know the sources of all things, was applied to the same study. Thus Greek geometry was not

only a consideration of the properties of straight lines and circles, but much more, a development of those properties by processes of thought from axioms laid down as a basis. If the modern mathematician can see defects of logic in much that has been handed down, he still follows Greece in the method of thought by which he deduces one result from another. Those same methods, indeed, are used to criticize the defects of the early work and that he is able to do so is chiefly due to the extended range of ideas which the developments of all forms of science have been able to give him. Some of the great names of antiquity in what we now term the humanities, are also great names in the history of science; Plato and Aristotle played no small part in the early developments.

The Greek school appears to have started about the seventh century before Christ, and it lasted nearly a thousand years, coming to an end finally after a long struggle against the stifling effect of the Roman conquest. For the first three hundred years Greece, a free nation, or at least under the government of its own citizens, found leisure to devote to intellectual pursuits, as a host of great names testifies. The invasion and repulse of Asia in the fifth century B. C. seems to have quickened rather than retarded the development of literature, science and the arts, perhaps under the spur of the construction of a single nation with democratic ideals from numerous small states. The conquest of Greece by Alexander in 330 B. C. transferred the sceptre nominally to Egypt through the enlightened policy of the Ptolemys who founded and encouraged schools of learning in Alexandria. But those who taught and worked there were all under Greek inspiration, and most of the advances were made by those of Greek origin. Apparently the foreign soil and alien patronage, however, gave only a temporary lease of life, for a decline started soon after and was accentuated in the first century B. C. when the Romans conquered Egypt and gained command of the whole civilized world. They themselves contributed little. The Fall of the Roman Empire through the incursion of the northern tribes, the rise and spread of Mohammed's followers, who, it is true, brought in other sources of knowledge from the eastern world but contributed little themselves, the domination and repression of free thought by ecclesiastics, left no opportunity for schools of science to grow. But few names appear in this period and those who sought to study the problems of nature had little opportunity to extend the borders of knowledge.

The first signs of awakening appeared towards the end of the fifteenth century. Revolts against ecclesiastical authority in England by the reigning sovereign, and later in Germany by Luther, the great geographical enterprises which soon resulted in a knowledge of the principal land areas of the earth, the curbing of royal ambitions by

the defeat of the Spanish Armada, and the brief interlude of a commonwealth in England, had their counterpart in the invention of printing and the appearance of scholars advancing knowledge in almost all the civilized countries of the western world. The tremendous steps taken in the course of two centuries, culminating in Newton and Leibnitz, finally placed scientific investigation on a plane where it became largely indifferent to what was going on in the political world, and where it was able to pursue its own course, hampered it is true, by wars and revolutions, but never without great names which will live as long as the history of scientific development is remembered.

This rapid sketch may serve to assist in seeing how the progress of scientific thought has been related to the development of civilization. We are all apt to regard our own concerns as independent developments and too often the history of science is treated in the same way. But in looking at the subject over long periods of time, we should treat scientific development as one of the phenomena which will illustrate progress or decline. The attitude of mind which leads to a search for the secrets of nature is simply one manifestation of a common desire for progress and if so, we should see signs of this desire in many directions. The ultimate causes which underlie the changes which have taken place—which produce periods of activity and inactivity in a whole people or group of peoples—are unknown and will probably only be finally found in the laboratories of the biologist and the physicist. All we can do now is to correlate the facts as far as possible and record them for future use.

Let us return to the Greeks and examine a little more in detail their contributions. In doing so we are at once faced by the difficulty that most of our knowledge comes to us second hand and in the form of treatises which gathered together past achievements. But few names survive and it is not always easy to apportion the credit. It sometimes appears that a name represents a school rather than an individual. This is certainly true to some degree of Pythagoras, to whom is usually credited the theorem that the sum of the squares on the sides of a right-angled triangle is equal to that on the hypotenuse. He certainly formed schools, but these were conducted as secret societies, the members of which might not divulge the knowledge they attained. Their continued existence for a long period of years was probably much assisted by the custom or rule of attributing all discoveries made by the members to their founder, thus avoiding much heartburning and jealousy. Nevertheless Pythagoras seems to have been responsible for placing geometry on a scientific basis by investigating the theorems abstractly. Briefly stated, it may be said that up to the middle of the fifth century B. C., shortly after the battle of Salamis, the Greeks had discovered the chief properties of areas in a plane and most of the regular solids.

Their clumsy notation for numbers handicapped them in arithmetic, but they knew such properties as that the difference of the squares of two consecutive numbers is always an odd number, they defined progressions of different kinds, and they had some acquaintance with irrational numbers. This was perhaps the first school which devoted itself to investigation for its own sake without any special reference to possible applications to physical problems.

Then followed the golden age of Greece with Hippocrates of Chios, Euclid, Archimedes, Appolonius, Hipparchus, and a number of others less well known. Plato and Aristotle must also be included for their contributions to the forms of logical reasoning which should be adopted, and Plato in particular contributed in this respect to an extent which has lasted until our own times. Euclid, whose personality is decidedly nebulous, wrote a text book on the geometry of the line and circle by which most mathematicians for the next two thousand years were introduced to the subject: it is indeed only during the last few decades that it has been replaced by modern texts. Appolonius did much the same thing for the curves of the second degree—the ellipse, parabola and hyperbola. While we know little of Euclid's own contributions as a discoverer, it is fairly certain that Appolonius had not only mastered all that was previously known, but greatly extended that knowledge himself. But of all those who lived up to the time of Isaac Newton, there can be little doubt that Archimedes is the chief. He is recognized as the founder of mechanics, theoretical and practical. His work on the lever alone entitles him to fame: "give me a fulcrum and I will move the world." He initiated the sound study of hydrostatics, advanced geometry by discovering how to find the area of a sphere and that cut off from a parabola by a straight line. He also discussed spiral curves, finding many of their properties. In arithmetic he seems to have had methods for dealing with very great numbers similar to those we now use by the index of the power of ten which can represent the number. These brief statements are only smbolic of what he achieved. His activities were very extended, and he preferred the modern custom of writing essays, or memoirs as we now call them, rather than treatises or text books. The tradition runs that he was killed at the fall of Syracuse to the Romans because, absorbed in a geometrical diagram, he insulted the Roman soldier who was spoiling it.

Hipparchus was mainly an astronomer, and indeed one of the founders of the art, and it was in the course of his work that he initiated Trigonometry as an aid when angles had to be measured as well as lines.

During the following centuries up to the Fall of Rome in A. D. 476, there were no great advances, the principal name of the period

being that of Diophantus, whose main contribution was a study of indeterminate equations. Algebra seems to have been developing very slowly and naturally, first by abbreviation of the words of a statement, next by a typical word (heap) and later by a symbol to represent the unknown, and finally by the adoption of symbols for common operations, like those of addition, equality, and so on. Diophantus took a great step in this direction, and he may be regarded as the father of Algebra in the main stream of the development of mathematics, in the sense that he placed it on a basis which rendered it capable of development. However, his work received no recognition at the time and was not revived for more than a thousand years.

We must now say something about some of the tributaries which paralleled the main Grecian stream and connected with it through the Moorish invasion of Europe during the 7th and 8th centuries. During the time of their dominance the Romans, if they contributed little, at least allowed development to continue in the centers of civilization. The Arabs, however, following the example set by the theologians, who by this time were beginning to come into power, had little use for scientific works and investigation, and between them they destroyed completely the greatest library and museum of antiquity, that of Alexandria, so that most of the written learning of that and of previous ages was lost for all time. They did however form a valuable connection with the Hindu mathematics of their time and appeared not only to have brought it to Europe, but to have assimilated it themselves to some considerable extent.

The origin of Hindu mathematics seems very uncertain. Apparently some centuries before our era, they had developed arithmetic further than the Grecians of the same period, but we do not know to what extent the Persian conquest might have been responsible for introducing early Greek learning into India. It is however true, that from whatever source they started, they developed arithmetic and geometry from the mensuration point of view further than the Greeks who had considered geometry from an abstract standpoint. In illustration of this, it may be mentioned that they had found good approximations to $\sqrt{2}$ and to $\pi$. But their greatest contribution is our present number system, which came to Europe through the Arabs and hence got its name. From the seventh century it does not appear that they formed an independent school. Hindu mathematics seems to have been largely improved by the needs of astronomy. Their greatest exponents were Brahmagupta, who lived in the seventh century A. D., and Bhaskara, some five centuries later. Their work, again, is chiefly arithmetical. While the so-called arabic numbers were probably used by the former, the latter was the first who is known to have given a systematic treatment of the decimal system. Although the Arabs dispersed the western

schools, they set up schools of their own which developed mainly on algebraical lines. One name stands out prominently, that of Alkarismi —who may be regarded perhaps as the founder of modern algebra— the name of the subject itself comes from him. But he used no complete symbolism, and he and his successors developed it mainly from the arithmetical point of view. In this school the modern names of the trigonometrical functions appeared although little was done in the way of development.

Chinese science seems to have started about the same time as that of Greece, but to have had little or no connection with our western science until the sixteenth century. Whatever mathematics the Japanese had probably came from China. It would appear at first sight that the earliest developments arose independently in all civilized nations about the same time, speaking broadly, but our knowledge is so scanty that we can only say that the records nearly all date back to similar periods in each case. Whether there is anything significant in this fact must be left to conjecture.

With this brief sketch this era may be thought of as closed in so far as the development of science and particularly mathematical science is concerned. It witnesses a real beginning in the study of geometry and algebra, and to a much less extent, of physical principles. A system of logical reasoning was discovered which, in its main outlines, still forms the basis of all deduction at the present day. The properties of the simpler geometrical figures had been studied and committed to writing. The advance of arithmetic was hindered by a poor numerical notation, but the foundations were laid for future development by the introduction of the Hindu symbolism. Algebra had started to emerge from the rhetorical form of discussion into the more terse abbreviations which we now use. Astronomy never failed to have exponents, though many who doubtless desired to increase their knowledge were restrained by the superstition of the age and by ecclesiastical authority, which attempted to dictate the thoughts as well as the actions of men. In mechanics, Archimedes seems to have stood almost alone, largely, perhaps, because no scientific method in dealing with the fundamental problems of nature had yet become current amongst the learned men.

It is difficult to sum up in a phrase the reasons for the scientific hiatus which occurred during the following three or four centuries. The revival of learning which sprang up towards the close of the eighth century was almost barren of progress in mathematics. The schools established by Charlemagne, while teaching mathematics, developed interest in other directions. Reverence for authority appears to be the basis of nearly all the learning of the age and there is no more stifling attitude of mind for progressive evolution of ideas. We may attribute

this to a variety of causes any one of which may seem sufficient to explain it, but in reality, not one of them explains anything. It may perhaps be best likened to one of those pauses which nature seems to demand and in which breath is sought before taking the next forward step.

The first sign of activity found expression in the Crusades undertaken to free Jerusalem from Mohammedan control. Soon after this time, that is, at the beginning of the twelfth century, several of the modern universities were founded, partly as gatherings of students to learn and discuss, and partly as developments of schools which had been under the charge of the monasteries. The old Greek works on mathematics were revived and the learning transmitted by the Arabs began to be assimilated and taught. Systematic instruction and the writing of treatises for the transmission of knowledge were begun. These were mingled with courses on astrology, alchemy, and magic, perhaps, we may conjecture, because the learned man had little chance to earn his living except through supplying what was in common demand, and perhaps also because the ancient learning gave little enough scope for cultivation of the imagination.

A century later Roger Bacon appears on the scene. It is difficult to overestimate the man who, travelling over Europe and studying in Paris and doubtless absorbing the learning of his time, becoming a Doctor in Theology and probably a monk, could break away from his training and absolutely reject the ideas and methods of his age. One who could write that in order to learn the secrets of nature we must first observe, that in order to predict the future we must know the past, must certainly have had unusual clarity of vision. He taught too that mathematics was the basis of all science, but he clearly recognized that it could not replace experiment and knowledge of phenomena, but could be used to great advantage in deducing results when the phenomena had once been observed: the point of view is thoroughly modern. But his three great works failed to have much influence on his times and seem to have been forgotten for several hundred years. Like many of his predecessors and followers in science, he suffered for his opinions.

The real start of modern science opens, as has been mentioned earlier, in the middle of the fifteenth century, and from this time on there is no break in the sequence of scientific discovery. We have also less need to relate its progress to that of the social order. While there have been periods in which wars have seriously disturbed the ability of nations to produce and to foster learning, there has been no time in the period in which the whole of the civilized world was so far involved in struggles that intellectual progress came to a stop everywhere. Moreover, all attempts since 1450 to enslave the world, either physically or spiritually, have ended in failure. It is true that five hundred years is comparable with the periods of Greek, Roman, and Mohammedan

supremacy, but in each of these civilizations we see signs of decline in a far shorter period. At present there appears to be no indication that the crest of the wave of scientific progress has been reached. The world war just concluded, in its essence a struggle for liberty of thought and action, was far too short to submerge the knowledge of the existing generation and prevent it from being handed on to the next.

The period can be roughly divided into two parts, the two centuries until the time of Newton forming the first of these. It is chiefly characterised by a sustained effort to lay solid foundations for all those sciences in which mathematics is needed for development, and success very nearly complete was attained at the end of the seventeenth century. In Astronomy and mechanics, Copernicus placed the sun in the center of our solar system. Kepler, building on the observations of Tycho Brahe, gave the laws of the planetary motions. Galileo laid foundations for the study of mechanics and proved the rotation of the earth about its axis, besides making a host of astronomical discoveries with his telescope. In mathematics, the arabic numerals came into full use and were applied to the needs of daily life. The symbolism of algebra was completed in nearly the modern form used in elementary mathematics: much of the work done by Vieta, Cardan and Tartaglia involved the solution of equations of the third and fourth degrees. Trigonometry, needed by the astronomer, the map-maker, and the navigator, was developed so far that Rheticus calculated a table of natural sines to every 10 seconds of arc and to 15 places of decimals. Logarithms were invented by Napier, and seem to have been adopted universally almost immediately. Descartes invented analytical geometry and thus gave to the investigators of space-forms a new weapon of immense power, while Desargues laid the foundation of projective geometry. Fermat, an amateur and perhaps as able as any of his contemporaries, laid down many of the laws of numbers and founded the calculus of probabilities.

The brevity of this summary is not a measure of the achievements of these two centuries. For this we must look to those which followed. While judged by modern standards, the actual progress seems small, it was far beyond that which had been achieved in all the previous ages. The only earlier contribution which has stood the test of time is perhaps the geometry of the Greeks, for the work of Archimedes, especially in mechanics, seems to have remained almost unknown up to the time of Galileo. The period showed not only a sound laying of foundations, but in its development of ideas, often only dimly expressed, showed that the germination of the seeds of future knowledge had already begun. Progress was frequently hampered by an unfortunate form of rivalry in which a discovery was kept back so that its possessor could propose problems to confute the claims for knowledge of his

contemporaries. On the other hand, the circulation of knowledge was greatly increased by the possibility of printing old and new work. Books became regular articles of merchandise and could even be picked up in out of the way places.

The era of Newton is so important in the history of both pure and applied mathematics that no excuse is necessary for dwelling on what was achieved. If an attempt be made to characterize its results in a single sentence, it may be perhaps best emphasized as the epoch of the discovery of the fundamental laws of continuously varying magnitudes. Before this time such solutions of dynamical problems as had been obtained were isolated. Newton's formulation of the laws of motion and proof of the law of gravitation were found in his hands and in those of his successors sufficient to deal with all the problems of physics which were then and later under consideration. Little progress, however, could have been made without the necessary complement, the calculus, which permitted of the study of varying quantities by symbolic methods. Rates of change, when uniform, presented little difficulty; the real problem was to deal with them when they were variable, as, for example, in the motion of a pendulum or the vibrations of a string. To a limited degree, the geometry of the straight line and the circle can deal with these questions as Newton showed in the classic translation into geometry of his results for the motions of the moon. But his manuscripts indicate that he failed beyond a certain point to give geometrical proofs of other results obtained by means of the calculus.

But as I have emphasized before we must not only look to the applications, the chief question in Newton's time, but also point out that varying magnitudes have been studied for their own properties so extensively that they form the larger part of mathematical developments up to the present day. If we except the science of discrete numbers, it is only in comparatively modern times that discontinuous magnitudes have received any extended study and even these have been advanced in many cases through developments of the calculus.

Isaac Newton seems to have been one of those very rare cases of genius breaking out without any very obvious stimulus from a particular teacher or school. His first introduction to mathematics was accidental: a book in astrology picked up at a fair in 1661 containing geometry and trigonometry induced him to study Euclid and then to continue by reading such text books as were available. His discovery of the calculus or "fluxions" as he called it, was made within three years and a half, the binominal theorem was formulated about the same time, and a year later he began his first attempts to prove the gravitation law. He was elected Professor of Mathematics in 1669, eight years

after his entry into Cambridge as an undergraduate. At this time his chief subjects for lectures and investigation were optics and algebra, the former of which involved him in much controversy. In fact all through his active period of work in mathematics, he seems to have suffered from the difficulties of having his great advances understood and accepted, although there never seems to have been much question amongst his contemporaries as to his wonderful powers. In 1679 he discovered the law of areas and showed that a conic would be described by a particle moving round a center of force under an attraction which varied inversely as the square of the distance. But it was not until 1684 that he began to work seriously at gravitation problems, his first step being to show that a uniform sphere exerts the same attraction as a particle of the same mass placed at its center. From this time on progress was rapid. Within two years the manuscript of the Principia was finished and the following year printed.

The Principia, like most of the works of the time consists partly of results previously known, but by far the larger part of it is Newton's own work. It begins with definitions, the formulation of the three laws of motion and the principal properties which can be deduced from them, with some examples, forming an introduction to the first book which contains his main work on gravitation. The second book is chiefly devoted to hydrodynamics and motion in a resisting medium and the third to various applications of the first book to bodies and motions in the solar system. As stated earlier, the proofs are cast into a geometrical form and freed from all traces of the method of fluxions which Newton had used to reach many of his results.

This great effort seems to have nearly exhausted his great powers for he produced little after its completion. In 1695 he accepted an appointment at the Mint and six years later resigned his chair at Cambridge. He was only forty-five when the Principia was published and he lived for thirty-eight years afterwards.

The half-century which followed the publication of the Principia seems to have been mainly occupied in understanding and digesting the advances made. On the continent, where Leibnitz' notation for the calculus was mainly adopted, a firm foundation was laid for the progress which began in the middle of the eighteenth century. In England the reverence for Newton was so great and separation from the continent so effective, that his methods dominated all the work for nearly a century and a half. This was perhaps mainly due to the translation of the Principia into geometry which ensured its early acceptance but must have fostered a distrust of all results which could not be proved in the same way. And further, Newton's notation for fluxions, which did not bring out their essential properties, had great influence in preventing advances as against that invented by Leibnitz. Nevertheless

there are certain names in the English school which have lived to the present day. Brook Taylor who was born in 1685 gave the fundamental series for the expansion of a function which is known by his name, followed a little later by Colin Maclaurin with the particular case which is named after him. The latter also determined the attraction of an ellipsoid and introduced the idea of equi-potential surfaces. De Moivre, of French ancestry but English birth and training, introduced the use of the imaginary into Trigonometry and thus prepared the way for the great development of the theory of functions of a complex variable which took place in the nineteenth century. Roger Cotes must also be mentioned if only for the high opinion Newton had of his abilities: he was only thirty-four years old when he died.

On the continent the Bernouilli brothers, friends and admirers of Leibnitz, were largely responsible for realising the power of the calculus and making it known. They applied it to many physical problems but perhaps their greatest influence came through their teaching abilities. Most of those in this period who achieved distinction were their pupils and several of their descendents were well known as mathematicians during the eighteenth century. But the most able men of this period were undoubtedly Clairaut and d'Alembert. The former produced the first theory of the motion of the moon developed from the differential equations of motion: in it he showed that the theoretical motion of its perigee, which in the Principia was obtained to only half the observed value, agreed with observation when we proceed to a higher approximation. There was an interesting development in this connection. Clairaut at first thought that it would be necessary to make an addition to the Newtonian law in order to produce agreement and it was only when he carried his work further that he saw such an addition to be unneccessary. A similar addition was examined by Newcomb and others as a possibility which might explain the deviation of the perihelion of Mercury from its observed value, and it is only within the last five years that such an addition has been deduced from the relativity theory by Einstein. Clairaut also obtained the well known formula for the variation of gravity due to the shape of the earth.

D'Alembert is best known by his work on dynamics in which he showed how the equations of motion of a rigid body could be written down: the principle is still known by his name. He also reached the well known wave-equation, a partial differential one of the second order, in several physical investigations and showed how a solution may be obtained. This was pioneer work but its further development was not carried forward to any extent by him.

The great period of continental activity which began in the middle of the eighteenth century and contained the names of Euler, Lagrange,

Laplace, and many others of whom a brief mention only can be made, was characterised also by social ferment which has profoundly changed the basis of society. The French revolution with all its far reaching consequences took place in the middle of the time of greatest mathematical activity, and the Napoleonic wars, carried into almost every country of Europe, served to stir up intercourse between the nations to a degree which must have had much effect on all forms of scientific activity. At the same time, the American revolution produced a new body of political thought which has had its development in the formation of a great and powerful nation. In England, comparatively free from invasion or social disturbance, little was produced of permanent value, the influence of Maclaurin being directed to the retention of published Newtonian methods. With the single exception of the Italian Lagrange, the great names of the period belong to France and Switzerland.

Those who have had the most far reaching influence, judged by modern standards, besides Euler, Lagrange, and Laplace, are Legendre, Fourier, Poisson, Monge, and Poncelet. I omit those whose chief labors were more closely associated with experimental work. The means for publication in this period were becoming more extensive and it is less easy to discover what each man owed to personal meetings with his contemporaries. The principal academies of Europe were starting or had started their volumes of transactions, extended treatises could be published and circulated, and the scientific men had new opportunities to meet and discuss, even to some extent with those of other countries, especially on the continent. The more enlightened courts sought the services of the ablest scientific men and, when the latter did not mingle too much in politics, on the whole treated them well. Fortunately, at least for the leaders, their teaching was usually confined to small bodies of earnest students and they appear to have been little hampered by demands on their time and energy for administrative duties. It is interesting to note that in a time of political disturbance which was perhaps comparable to that produced by the great war, the main foundations of modern mathematics, both pure and applied mathematics, were firmly laid.

Leonard Euler, born in 1707 at Basle in Switzerland and educated in mathematics by Bernouilli, was perhaps the most industrious of all his contemporaries and it is difficult to say whether his services to mathematics are to be judged best by his excellent treatises on analysis, including the calculus, or by his original memoirs on applied mathematics. In the former, he followed up all that was known at the time, recasting proofs and setting the whole in logical order. In the latter, he is best known for his formulation of the equations of motion of a rigid body, for a similar service in the equations of motion of a fluid

and for his work on the theory of the moon's motion. With respect to the last it may be said that every modern method of treatment can be found to have started with Euler. He continued his work to the end of his life in 1783 in spite of losing his sight some fourteen years earlier and his papers by a fire in 1777.

J. L. Lagrange was born at Turin in 1736 and was, like Newton, practically self-educated as far as his mathematical studies were concerned. It gives some insight into the comparatively small body of mathematical literature at the time he was seventeen years old and his own great ability, that an accident directed his taste for mathematics and that after two years work he was able to solve a problem in the calculus of variations which had been under discussion for half a century. At the age of twenty-five his published work showed that in ability he had no rival. Before his death, at the age of seventy-seven, he had left his influence on almost every department of pure and applied mathematics. His generalizations of the equations of mechanics have proved to be fundamental in all modern investigations and his applications to dynamical theory of the principle of virtual work and of the calculus of variations are now even more important than at any time in the past. The latter is applied not only to particles and rigid bodies, but also to fluids. To celestial mechanics he contributed several new methods in both the theoretical and practical sides of the subject in which such topics as the general problem of three bodies, stability of a planetary system, mechanical quadratures and interpolation are developed. In pure mathematics his lectures on the theory of analytic functions, afterwards expanded in treatises, form the basis on which later writers built. He also founded the science of differential equations by considering them as a whole rather than a treatment of such special equations as might arise in particular problems. And he contributed some important memoirs on the theory of numbers. His influence was undoubtedly much increased by a remarkable gift of exposition both in lecturing and writing: those who have read his Mécanique Analytique in which his most important dynamical contributions were placed will appreciate this fact. And this may be said independently of the simpler problem before him than has the modern mathematician with the enormous mass of past work which he has to consider and the selection which must necessarily be made.

P. S. Laplace, whose mathematical ability is unquestioned, was essentially an applied mathematician in that he devoted himself almost entirely to the solution of the problems of nature by mathematical methods. In general, he was not particular about mathematical proofs or logic, provided he could obtain results: in many respects he may be said to be the founder of the more modern schools of mathematical physicists. His most enduring work has proved to be in the theory of

attraction, especially gravitational, and its application to the solar system. He made potential a real and valuable instrument of analysis and discovery, working out many of its properties and applying it in various directions. The famous second order differential equation $\nabla^2 V = 0$, which is satisfied by every gravitational arrangement of matter, has been used as a substitute for the simpler expression of the Newtonian law of attraction and is especially interesting at the present time, since it may be regarded as the Newtonian analogue of the Einstein law. Laplace was the first to attempt a complete explanation of the motions of the bodies of the solar system or at least to formulate methods which could be applied for this purpose and his *Mécanique Céleste* remained the standard work of reference in this subject for a century. The theory of the development of the solar system from a primeval nebula which goes by his name and which was independently set forth somewhat earlier by Kant, has never been entirely rejected, although its supporters have often changed it almost beyond recognition while retaining his name in connection with their work. Its fundamental idea consists of the contraction of matter under gravitational attraction, but few now believe that the matter can produce planets and satellites by the throwing off of concentric rings as he supposed. What he did for celestial mechanics, he also achieved for the subject of probability, his *Théorie Analytique des Probabilités* being the classic treatise in which the whole is put on a sound basis and developed in various directions. It must be added that he gave many theorems and results in pure analysis but in most cases these were invented to solve physical problems.

Of the remaining names in this period, Legendre was essentially an analyst, his work being mainly in the theory of numbers, which few mathematicians of this time altogether left alone, in integral calculus and ellipitic functions, his treatises on these subjects being still consulted. Monge and Poncelet may be regarded as the founders of modern descriptive and projective geometry respectively. Fourier in his *Théorie Analytique de la Chaleur* enunciated the theorem for the expansion of a function in a periodic form which has had such immense value in the discussion of all periodic phenomena, and has now a literature of its own. Poisson's work in attraction is on similar lines to that of Laplace, whose natural successor he seems to be.

It is convenient to view the progress made in the nineteenth and twentieth centuries from two points of view: one, the development of the three great branches of mathematics, geometry, analysis and their applications to other studies; the other, the development of new ideas which have applications in many branches of mathematics. While it may be said that the former is more particularly characteristic of the

first half of the nineteenth century and the latter of the succeeding period, it would give a false idea of the method of progress to regard these as anything more than general tendencies. But the difficulty (mentioned earlier,) of conveying an understanding of the advances made in pure mathematics, even by one much more familiar with them than the writer, occurs in an exaggerated form in attempting a chronicle of the work of the past century. The task is far easier in applied mathematics because most of us have some acquaintance with the problems, and the ideas to be conveyed are not so far away from our every day experience. Consequently, in the former, I can do little more than point to a sign post here and there, occasionally indicate the course of the road which has been followed, or describe by an analogy or an example a result which has been obtained.

The older geometry which consisted in the investigation of figures which could be generated under some simple descriptive definition like lines, circles, or conic sections, was greatly extended by Descartes' invention of analytical geometry. In the latter an algebraic statement of the properties gave rise to various classes of curves which could be ordered according to the forms of algebraic statement. Their properties could be investigated with much greater ease. The way was opened also for the consideration of the different kinds of curves or surfaces which possessed some definite general property; the properties common to a given class of curves or surfaces; the relations which may exist between theorems in analysis and geometry; and so on. When the methods of the calculus were added, the range of investigation was again widely extended through its facility for dealing with the properties of tangents and curvature. The new results obtained were undoubtedly instrumental in stimulating investigation from the more purely geometrical point of view. The names of Desargues, Monge, and Poncelet have been already mentioned as the creators of the subject of projective geometry; their work was continued and, during the third decade of the nineteenth century, developed into a separate branch by Moebius, Plücker, Steiner and a host of writers who have followed them. Simple illustrations of the idea involved, are that of map-making in which we represent portions of the spherical surface of the earth on a plane, or that of the shadow of a figure cast by a ray of light. These simple ideas have been generalised by considering the common properties of figures which are projected according to some given law and more generally by correspondences between two or more figures. Another development is that of transformations which leave properties unchanged. It will be seen at once that measurements of actual lengths of lines or metric properties, as they are called, are not those which would ordinarily be unchanged by projec-

tion, but this difficulty was overcome by Laguerre, Cayley and their successors.

Differential geometry is in its essence a study of the properties of geometrical figures by investigating the properties of small elements of those figures. Such properties as curvature of a single curve or surface, and those which depend on classes, such as the envelope of a systems of curves, the surfaces which cut systems of surfaces at right angles are the natural subjects of investigation under this head. In 1828 Gauss published a memoir which immensely extended the range of this subject, by introducing new ideas which have been applied to such topics as the deformation of surfaces under given conditions and more particularly to those properties which remained unchanged by deformation. The subject is closely allied to many problems in physics.

Many futile attempts to deduce the axioms of parallels from the other axioms laid down by Euclid led Lobatchewski and Bolyai to see what would happen if a geometry free from this axiom were constructed. The results showed that it could be made quite consistent in all its theorems, that some of the Euclidean theorems would still hold while others would be changed or generalised. Their chief successor was Riemann who showed that all previous geometries were special cases of a more general system. In our own time the subject has been developed in the direction of finding the properties which are possessed by the different geometries and also by investigation of the different sets of axioms which can be used as a basis for constructing different classes of geometries. In the applications to physics the most important has been perhaps the recognition that our own space is not necessarily Euclidean and that we can only find out its nature by examining properties which we are able to observe and measure.

The new developments have not prevented further research on the older lines. Geometry is still much used as a convenient language for the development and expression of analytical results. As seen below, the plane is the home of the complex variable, but in the theory of functions of this variable, it has become many-storied with ladders reaching from one story to another. Most of our physical problems, however, demand the use of three dimensional space and here the complex variable is not sufficient because with our ordinary algebraic rules, there are only two different kinds of numbers, the real and the imaginary, which are used to deal with two different directions in a plane. Hence the theory of vectors which deals with straight lines in any number of dimensions and particularly three has been evolved and is coming more and more into use on account of its brevity and compactness. It requires, however, a new notation to be learnt and a certain degree of familiarity with the operations which are possible.

The older theory of numbers in general dealt with integers and to a less extent with fractions, square roots, etc., that is, numbers which could be formed out of the integers by the ordinary operations of arithmetic. Gauss opened the way to a more extended idea of the meaning which might be attached to numbers by introducing those which are the solutions of an ordinary algebraic equation of any degree, whose coefficients are rational; such numbers are called algebraic. They naturally introduced complex numbers and have properties such as divisibility more extended than those which play a large part in our ordinary number system. This soon demanded a theory of congruences which, in their simplest form are numbers which, when divided by a given factor (called the modulus), always leave the same remainder (a residue). The theory of forms was another development which arose. Since Gauss' time the ideas have been greatly extended to many other kinds of numbers in which special classes have special properties, and these classes are the main subjects of investigation.

The extensive development of the theory of functions of a complex variable is perhaps the most significant achievement of the last hundred years. The imaginary was always arising in such questions as the solutions of quadratic equations and in the new branches. The next step was to give a geometric interpretation of a complex number by showing that it could represent in a single symbol the two co-ordinates of a point in a plane. A function of a complex variable was therefore a function which could take values over an area as against one of a real variable which could only take values along a line. The majority of functions become infinite for one or more values of the variable and these infinities play the major part in the development of the theory. To Cauchy belongs the honor of starting the work in the third decade of the nineteenth century, examining such questions as the possibility of developing such functions in series, their integrals, and the actual existence of functions of different kinds. Closely following him, Weierstrass and Riemann developed Cauchy's ideas, the former by basing his arguments on a special form—a series of powers of the variable—and the latter by using geometrical and even physical ideas for progress. Their successors have merged these different modes of development and have continued to investigate with success the representation of a function under given conditions, and the limitations of a given function. At the same time special functions, particularly those known as elliptic, were being developed by Abel and Jacobi, and the latter extended them in a very general manner. We have now many groups of such functions, some of which have become of sufficient importance to have whole treatises devoted to them.

The study of functions of real variables during the past half century has been largely due to the critical spirit which has compelled

mathematicians to examine thoroughly the foundations of the calculus. It consists of "all those finer and deeper questions relating to the number system, the study of the curve, surface and other geometrical notions, the peculiarities that functions present with reference to discontinuity, oscillation, differentiation and integration, as well as a very extensive class of investigations whose object is the greatest possible extension of the processes, concepts, and results of the calculus." (J. Pierpont, Bull, Amer, Math. Soc., 1904, p. 147).

Ever since the invention of the calculus, the relations between two or more variables which contained also their derivatives and known as differential equations, have been continually brought before the eyes of the mathematician by the physicist, owing to the fact that the simplest symbolic statement of nearly all physical problems has been in the form of one or more differential equations. For finding the derivatives or integrals which arose in his work, definite rules were generally available even if they demanded much calculation. But he failed to find such rules for most of his differential equations, and in fact they do not exist. The pure mathematicians, led by Cauchy, took up the question from other points of view asking, in particular, the nature of the function which is defined by a differential equation. This is naturally an extension of the theory of functions and the methods of the latter opened the way. But the questions are so difficult that only a particular form, known as the linear, has made any considerable progress; this form, however, does embrace a large number of the functions whose properties had been examined. In our own generation the subject has been extended by the consideration of equations in which integrals also occur; these again are necessary in certain physical problems.

Most of the progress which has been made in applied mathematics will be treated in the article on Physics, but in addition to the remarks at the close of this article some few words may be said of those branches in the development of which mathematics plays the larger part. The chief of these is the dynamics of a system of particles and rigid bodies. W. R. Hamilton and C. G. J. Jacobi, in the second quarter of the century, put the equations of motion of all such systems into forms which not only permitted of remarkable generalisation, but indicated new methods of integration which opened out research into the general properties of such systems. The later work has been mainly developments and applications of these methods. The particular branch of this subject known as celestial mechanics has been continued on the practical side by extended theories of the motions of the planets and the moon and on the theoretical side by investigations into the general problem of three or more bodies. In the former numerous writers have continued with increasing accuracy the work of

the eighteenth century: for the latter new foundations were laid by Poincaré some thirty years ago.

Hydrodynamics has had less success in its applications. The prediction of the tides, chiefly from the work of G. H. Darwin, and the relative equilibrium of liquids in rotation by him and Poincaré have advanced in a satisfactory manner, but the knowledge of the motions of bodies in actual fluids is still in an elementary condition, especially when an attempt is made to apply it to under-sea and air craft. This, of course, refers, not to the experimental side, but to developments from the equations of motion. An interesting, but now almost neglected subject is that of vortex rings in a perfect fluid, the main features of which were given by Helmholtz and Lord Kelvin, giving rise to a hypothesis, now abandoned, that the fundamental atoms of matter consisted of such rings. The motions of our atmosphere have so far defied attempts at explanation on any general plan. The theory of sound, on the other hand, in the hands of Helmholtz and Lord Rayleigh, has been well developed. The theory of elasticity is almost entirely a creation of the present century and has found many applications.

Many of the ideas which are now fundamental in mathematics have had their origin in an attempt to advance some particular branch. Development has proceeded to a certain stage by means of known methods and then stops, owing perhaps to mathematical difficulties or to a failure of those methods to cast further light on it. A new method of attack has then been evolved, showing new roads by which it may be explored, after a time leading to openings which enable the investigator to continue on the earlier lines. These new methods have then been seen to be applicable to various other branches, thus forming connecting links and shedding new light. Indeed, one not infrequently meets with a statement that all mathematics can be based on some one of these ideas. This may be true, but progress demands that the subject be cross cut in many ways: a new country may be opened out by one great highway, but it is only well developed by several main roads in different directions with numerous connecting branches. I shall take up certain of these ideas and try to indicate briefly their bearing on various mathematical topics.

Some illustrations have already been given of the effect which a critical examination of the logical processes used in mathematics has produced. In geometry, the examination of Euclid's axioms has led to the discovery of ideas of space other than those which were current in earlier times. In analysis, algebras have been constructed in which some of the familiar rules have been dropped or changed. The way was thus opened to the examination of the foundations on which mathematics rests. Here the work gets close to a consideration of the mode

in which the human brain can think. But without going into this question it is possible to indicate the general method followed at the present time. At the outset a set of statements, usually called axioms or postulates (there is a difference of opinion as to the exact meaning to be attached to these words), is made. These statements may be redundant but must not be contradictory: a complete system is one which contains all the statements necessary for the object in view but which has nothing unnecessary or redundant so that no statement or combination of statements can result in another of the set. To connect and deduce, a system of reasoning is also required. From this it will be seen that mathematical science has no necessary relation to natural phenomena, but that it can be regarded as solely a product of the brain and that its results are simply consequences which may be deduced from ideas without external assistance. The last half century has seen great progress made in clarifying our ideas and in the introduction of rigorous methods of argument. It has now extended to the phenomena of nature, particularly in the direction of a reconsideration of our ideas of time and space and in an examination of our powers of observation, as will be illustrated below.

The idea of invariants has permeated every branch of pure and applied mathematics. In its elementary forms it is not difficult to understand. Natural processes are subject to change but we can nearly always find certain features of them which seem to remain the same in the conditions under which we observe them, as for instance, mass and energy. In geometry, the ratio of the circumference of a circle to its diameter is constant, the ratio of the sections of any system of straight lines cut by three parallel lines is the same, certain properties of a system of curves remain unchanged under given conditions of deformation, and so on. In analysis, one of the commonest modes of investigation is to find out what expressions remain unchanged when a specified change is made in the symbols. It has been said that all physical investigations are fundamentally a search for the invariants of nature. The various terms which occur in modern algebra, such as discriminant, Jacobian, covariant, are special forms of the same fundamental idea.

The idea of permutations and combinations which few of us fail to meet with in our every day experience, was chiefly developed in earlier times from the point of view of the number of arrangements which could be made of a set of objects under certain specified conditions. The modern theory of groups is the natural successor of this subject, but as has so often happened, the point of view and its development have changed. If we have a set of symbols and replace one by another according to a specified law, we can consider what changes will leave unchanged certain combinations of those symbols, as well as

the number of such changes which can be made. In doing this we naturally make a connection with the theory of invariants. Such a set of changes is called a group. But a more fruitful idea has been obtained by considering what combinations amongst the symbols, using a specified law of combination, will always produce another of the symbols, and will produce nothing else. As a simple and familiar example, take the series of numbers 0,1,2, . . . with the law of addition. If we add any two of these numbers we always get another of the same series and we never get any other kind of number. The whole of the series is called a group from this point of view. The even numbers form a sub-group under this definition but the odd numbers do not because the sum of two odd numbers is not an odd number. If we use the same series, omitting the zero, with the law of multiplication instead of that of addition, we again have a group, but now the odd numbers form a sub-group. Again we may consider the operation of turning a straight line through 60° in a plane about one end of the line. There are obviously six positions of the line and in whatever one of the six positions we start, a turn through 60° will always give one of the other positions. The whole set constitutes a group.

The idea of a group of substitutions enabled Gabois and Abel, about the middle of the nineteenth century, to open up the way to treat algebraic equations of a degree higher than the fourth and in fact to show that the methods used to solve equations of the second, third and fourth degrees could not in general be applied to those of the fifth and higher degrees. The quintic had long been a puzzle to mathematicians, all attempts to give a general solution in terms of radicals having failed. Later on, Sophus Lie applied the idea of groups to the solution of differential equations and was able to indicate the nature of the solutions in certain general classes. Before his time the methods for finding them had been disconnected and apparently without any common property. Another form of the group theory has been applied with success to the investigation of curves and surfaces and it is not too much to say that the idea has been one of the most fruitful in producing progress. "When a problem has been exhibited in group phraseology, the possibility of a solution of a certain character or the exact nature of its inherent difficulties is exhibited by a study of the group of the problem."[1]

One of the most useful efforts of the nineteenth century mathematicians has been in the direction of proving the possibility or impossibility of performing certain operations or of solving certain problems, that is, in the investigation of existence theroems, as they are often called. The squaring of the circle or the trisection of an angle are two of the oldest of them and later arose the obtaining of a finite numerical expression for the number $e$ which is the base of the Napier-

ian system of logarithms and which arises in numerous mathematical and physical investigations. The failure of attempts to solve these problems finally led mathematicians to consider whether they could not be proved to be insoluble under the conditions laid down. Complete success has rewarded them. We now know that with the use of the ruler and compasses alone, it is not possible to find a square which shall be exactly equal in area to that of a given circle, nor given any angle is it possible to construct the lines which shall divide it into three equal parts. The number $e$ too cannot be exactly expressed by fractions or square roots or any other such simple numerical representations, though it can be approximated to as closely as we wish by decimals or in other ways. The labor of useless effort on the part of the mathematician is thus avoided, though we shall still probably continue to hear of those who claim to have performed the impossible. In our time it is quite usual as a part of an investigation to find included in the construction of some new function or in a new representation of a known function, a proof of its existence; especially in those cases where the possibility may be called into question. In celestial mechanics an important part of Poincaré's work consisted in proofs of the existence or non-existence of different kinds of motion and of different kinds of integrals. Indeed we have a considerable class of literature which consists solely in demonstrating the existence of functions or curves with little indication of the methods by which they may be constructed. The stimulating value of such researches in suggesting problems is often forgotten by those who, with some justice, complain of their dullness.

It is strange in connection with existence theorems that some of the problems, most simple in statement, are still unproved. Long ago Fermat stated that there are no whole numbers which will satisfy the statement that the sum of the $n^{th}$ powers of two whole numbers is equal to the $n^{th}$ power of a third whole number, except when $n$ is equal to 2. This impossibility has been proved for all values of $n$ up to 100 and for a few beyond, but no general proof has yet been given that it is universally true. Again, there is no general method which will enable us to pick out the prime numbers, that is, those which are not divisible by any other number except unity. In geometry we have the famous four-color problem in which it is desired to prove that a map consisting of countries of any shape and arrangement can always be painted with four colors so that no two adjoining countries will receive the same color. In these and similar cases, no exceptions to the statements have been found and there exist no complete demonstrations of their possibility or impossibility. It is of course assumed that if they are true

[1]L. E. Dickson, Paris International Congress, Vol. 2, p. 225.

a proof can be constructed without a change of our axioms concerning number or space.

It will be seen from the sketchy remarks of the last few paragraphs that at least one outstanding feature of pure mathematics during the last century has been its emancipation from the trammels imposed by any necessity for application to physical problems. It is, nevertheless, necessary to say a few words about these applications, although the major part of the story naturally comes under the history of physics. Under the general term "applied mathematics," are included at least three methods of study. In the first and simplest, we translate the physical problems into symbols and deduce the consequences we desire by mathematical methods. The work consists, therefore, of little more than an argument on lines laid down by the mathematician. In the second, a study of the formulae and relations which have arisen from physical problems is made, without any special desire to apply them to the phenomena: as indicated above, much of the pure mathematics arose in this way, even before it was recognized that such study was a quite legitimate intellectual exercise. In the third, the mathematical processes used by the applied mathematician are studied in order to find out their limitations, the extent of their validity, what extensions they will admit, how more general methods may be obtained, the best manner of treatment, and so on. This is not by any means an infertile source of progress, as may be illustrated by Poincaré's work on the divergent series which are used to calculate the places of the moon and planets.

The most fundamental change in the attitude of applied mathematicians has been in the recognition and working out of the consequences of simple fundamental principles or laws. Foremost amongst the latter is that known as the conservation of energy, brought into prominence in the middle of the nineteenth century by the labors of Helmholtz and Kelvin. It is now regarded as the chief invariant of the universe and has been applied to every branch of physics. Owing to the various forms which energy can take and to the fact that we are practically compelled, in applying mathematics to a physical problem, to deal only with some partial phase of it rather than with the whole, we cannot always assume that the principle holds in a particular problem. But in the majority of such cases the energy which is lost or changed from the particular form which we are considering is small so that this loss may be neglected or allowed for. When the loss is zero, the differential equations of the problem admit of an integral which expresses this fact. Newtonian mechanics lead to two forms of energy, kinetic (that due to motion) and potential (that due to position) and the development of the mathematics of all material systems has been mainly based on this separation.

The principle of Least Action, enunciated by Maupertius in 1744 but first put into correct mathematical form by W. R. Hamilton a century later, is essentially one which demands mathematical treatment. The Action of a system is a certain function depending on the velocities and relations of its parts which takes a minimum value whenever the system moves under natural laws. The process of discovery of this minimum leads to the differential equations of motion of the system and thus includes a complete statement of the problem. Since the initial form of the function is an integral, its mathematical treatment consists in finding the least value of this integral and thus becomes a problem in the calculus of variations to which considerable attention has been given by pure mathematicians, especially during the last two or three decades. While the physical consequences of the principle have been less developed than those of energy, there appears to be a growing feeling as to its fundamental importance and the aid of the mathematician in solving the problems which it raises, will become increasingly necessary.

The study of the properties of a system containing a large number of particles not fixed relatively to one another, now generally studied under the term, statistical mechanics, has penetrated into several branches. It is to be understood here that the question is not one of finding the separate motions of the various particles but to try and find out such properties of the system as can be deduced from averages. It is probable that as long as we cannot observe the motions of the separate particles, we should be able to deduce in this way most if not all the properties of the system that we are able to observe. Maxwell and Boltzmann founded the subject from this point of view, applying it to the kinetic theory of gases, while J. W. Gibbs was largely responsible for its application to thermodynamics. In astronomy the present century has seen it applied to the motions and positions of the stars, thus opening the way to a knowledge of the outlines of the construction of the stellar universe. Mathematically these questions are obviously very similar to those parts of probability which deal with errors of observation and thus form a continuation of the development of that subject.

The mathematics of continuous media has received very complete development during the century and, besides the earlier investigations into the motions of fluids and elastic solids, has been applied to the so-called luminiferous ether and finally by Maxwell to the whole electric field. In all this work the continuity of the medium is a fundamental axiom involving the hypothesis that no action can take place without its presence. Further, the Newtonian laws of motion were assumed as fundamental and time and length were regarded as unchangeable separate entities. The classic experiment of Michelson and

Morley which showed that the velocity of light was apparently independent of the velocity of the medium in which it travelled, and observations on the motions of certain particles with very high velocities, started a reconstruction of ideas. It was possible to explain the results on the assumption that the length of a body depended on its velocity. It was then that Einstein sought to generalize Newton's equations of motion by making them entirely relative, not only for uniform velocity, but also for accelerated motion. By adding the assumption that the laws of nature should refer to all such systems of reference and by making the velocity of light a fundamental constant of nature, he was finally able to generalize the whole subject. Matter appears simply as a form of energy. Gravitation can be exhibited as due to a warping of space without the introduction of force, but if this is so, the Newtonian law requires a minute correction. The motion of the perihelion of Mercury and the bending of a light ray as it passes near the sun have given remarkable confirmation of this theory. His work crosscuts several subjects which previously had an interest only for the pure mathematician, in particular the theory of extensible vectors (tensors) and the theory of invariants. His differential equations for the gravitational field should supply mathematicians with problems of great difficulty and interest for some time to come. Those for the electric field are unchanged. A further interesting product of this work on relativity lies in the question of what we can or cannot observe and in what may be deduced from observation without the assumption of hypotheses. This is, of course, fundamental in all experiments, but it has received little attention as an exact science. Given that we can only observe certain properties of a function, what limitations is it possible to make in the construction of the function?

Finally the quantum theory of Planck, according to which energy is not infinitely divisible but is always received or emitted in exact multiples of a fundamental unit, is bringing forward the necessity for a calculus allied to that of finite differences as against the differential and integral calculus which depends in general on continuity. It is even suggested that not only energy, but also space and time have ultimate parts which cannot be divided. At present the mechanics of this theory is in a very nebulous state but as a statement of the results of observation it has had very considerable success. The construction of the atom is now generally exhibited as a kind of minute solar system, but there is as yet no indication how such a system can only permit of the limited number of motions required by the quantum hypothesis. It may perhaps be due to fundamental instabilities for we know little of the ultimate stability of most of the motions in the problems of even three particles. In any case, the field of work has approached one of the oldest of mechanical problems and the reaction of celestial mechanics on that of the atom should prove stimulating to both.

# DEMOCRATS AND ARISTOCRATS IN SCIENTIFIC RESEARCH

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MOTIVES, methods, and enthusiasm in approaching and conducting scientific research certainly are as widely different as those in any other form of public endeavor. This is perhaps obvious enough to those accustomed to think in research terms. The public at large, it may be said, does not think in such terms, and has an indistinct notion as to what scientific research is. And this is not to be wondered at, for the public at large is not in the habit, not to mention the mood, of following scientific publications and so is largely dependent for its information upon a daily press, out of its depth entirely where science and exact truths are concerned. Much of the press knows a great scholar and gentleman, William Osler, chiefly, if not solely, for a chance, half-chaffing remark on longevity. In guileless simplicity it hails an untried adventurer sailing for southern seas in search of the "missing link" as a new Darwin, and it will continue in sublime ignorance of the stupendous amount of scientific material submitted to a critical world by that retiring but superhumanly able observer, to regard the distinguished biologist as the one original "monkey man" of all time.

Yet a public so educated, even with the movie scientist as the prototype of the species, dimly classifies research men into two groups. The one, alert to the present, includes the physician driving with all the power of his trained mind toward the achievement of a "cure" for one of mankind's maladies, and the engineer or botanist contriving methods which will make the desert blossom as a rose. In the other, we find an irresponsible person, perhaps damned with faint praise as an intellectual, given to investigating minute details of unimportant subjects, fussy and irritable if disturbed in his meditations or laboratory procedures, and prone to have beautiful if undeserved daughters, popular and addicted to the habit of staying out late nights. And after all, with some modifications, the classification holds, if one deserts the movies and views the latter creature on his own plane. It is this group, the science-for its-own-sake and knowledge-as-its-own-reward crowd, that we may designate as the aristocrats of science, in contradistinction to that other class, impatient, often as closely altruistic as it is given to man to be, who from their ardent endeavors in the direction of popular

demand and rigid dependence on the wishes of the *people*, may be labeled with the over used and much abused word democrat.

In the passing of William Osler and Emil Fischer of Berlin, science has in the past year lost two of the foremost minds of the century. Widely different in their fields of scientific endeavor as well as in their manner of cultivating them they may serve to illustrate anew the distinction drawn above.

Osler spent his life among people, giving to the full of his own strength and skill in the personal effort at the bedside, and outside as a great organizer. He was a member of innumerable societies and committees with the avowed purpose of promoting human welfare in one form or another. His objects are understandable to all. To heal the sick, to prevent suffering, to teach others to do the same, these are things that come within the range of common experience. He was a profound student of tuberculosis and every one knows what tuberculosis is, of typhoid fever, and diseases of the heart. A layman picking up his papers of twenty years ago on cancer of the stomach could see at a glance what he was driving at, might recognize the cuts, and with a little effort understand something of the pathology. His great textbook which went through many editions, has traveled far, being often seen on the "Science Shelf" in the smallest of village libraries. His name is familiar consequently to some of the most superficial of readers and because of the nature of his subject he is a person to them, where to the same individuals Fischer would be "a book."

The enthusiasm he aroused in his students is characteristic of the man. His own life-long student's attitude-that same spirit which used to lead him to say, "And now let us see where we made our mistakes," as he marched into the old autopsy room at Montreal-kept him on earth as far as his pupils were concerned and made him approachable to all. There was a very human touch to his work that belonged not merely to its nature but perhaps much more to his treatment of it. It must have been a primal urge, something essential in the man, which led him to devote himself to the people in such a way. His campaign must have been for end results, however interesting the preceding discoveries were of themselves. If he studied typhoid fever, in the last analysis it must have been with the object of preventing typhoid fever and making it unnecessary to study it, not for the mere acquisition of knowledge of typhoid fever for its own sake. Twenty years ago, commenting on the waste of life from this disease due to ignorance and neglect, he said, "Very different from death which comes with friendly care to the aged, to the chronic invalid, or the sufferer with some incurable malady, is that from typhoid fever. A keen sense of personal defeat in a closely contested battle, the heart searching dread lest something had been left undone, the pitifulness of the loss, so needless—

and as a rule in the 'morn and liquid dew of youth'—the poignant grief of parents and friends, worn by the strain of anxious days and still more anxious nights—these make us feel a death from typhoid fever to be indeed a Delian sacrifice. For fifty years the profession has uttered its solemn protest as I do this day; we have done more—we have shown how the sacrifice may be avoided and the victims saved." The marked decline in the incidence of typhoid fever in late years, due to the combined efforts of medical men, sanitary engineers, and public health officials, must have given Osler untold satisfaction.

But if William Osler was a public personage whose life played a part, great or tiny, in that of every one of us who has known what it is to be sick, what shall we say of the incomparable Emil Fischer, perhaps the greatest organic chemist who ever lived, whose place in the history of his science will be secure when Osler's in the development of his, is centuries forgotten? How many have heard of Emil Fisher? In his own group, chemists and biologists, he has for a generation sat on the highest tier of the seats of the mighty, and yet his place has been quite without the public ken. A supreme investigator who undertook the most fundamental and most difficult problems of his science, he carried on a labor which by its very nature has been inexplicable to the average man. To be sure the average man would not wish it explained to him, would resent it deeply if forced to listen to such dry stuff. But he is safe. No newspaper in the country would be competent to discuss it for him. The pathetic attempts of the daily press to keep up on the relatively simple subject of poison gas in the late war and the rude caricatures of chemical nomenclature which finally found their way to the proof reader and past him, merely serve to emphasize this point.

Let us see if, without being too technical, we can show how it was that Emil Fischer came to stand so high above the crowd. Forty-five years ago Fischer discovered phenyl hydrazine, an accomplishment which alone would have sufficed to give him some fame, but in the hands of its discoverer phenyl hydrazine became a tremendous weapon, a 42 centimeter gun, a whole park of them in fact, for a war on the ultimate nature of the carbohydrates. These last substances are of immeasurable importance, making up as they do the bulk of matter in the vegetable world, including the sugars, starches, wood and various intermediary bodies. With the different sugars, scores in number, into which all of these substances can be ultimately resolved, phenyl hydrazine yields characteristic crystalline precipitates, permitting relatively easy identification. Be it said, once and for all, however, that Fischer did not pack up his phenyl hydrazine and his polariscope and enter the sugar business, nor did his altruism lead him to desert theoretical fields and apply his trained mind to the production of cheap

sugar for the people. What Fischer was interested in was the composition, the structure of sugars, and to few men has it been given to penetrate the subject so deeply. Molecules of the sugars, like those of all substances, are composed of atoms which stand to each other in definite relations in space. The molecules, that is to say, the essential units of a certain group of sugars, all contain six atoms of carbon, twelve of hydrogen and six of oxygen, and yet the group includes several distinct substances because the arrangement of these atoms in space is different. For instance, one possible difference in arrangement is that in which the atoms of two molecules are so disposed that the three dimensional molecule of one is the mirror image of the other. When the likeness is so close the properties are identical, except for one feature. A ray of light which has been "polarized" by passing through a Nicol prism is bent to the right by one form, by the other to the left. With greater variation in the arrangement greater differences in chemical properties are noted. Abstruse matter this, and all theory, for no piling up of lenses has ever been sufficient to enable man to gain a faint glimpse of one of these hypothetical molecules. Yet the theory stands, and by virtue of the enormous number of predictions made upon its basis which were later realized by experiment. And to Fischer, using the sugars, is due much of the credit for the establishment of this, the so called Van't Hoff and LeBel hypothesis.

The study of sugars led him to the substances that in nature act upon them, the enzymes or ferments, substances like the diastase which acts upon starch converting it into glucose. That enzymes are specific in their action had long been known, but it remained for Fischer to demonstrate how high that degree of specificity is. To one of a pair of extremely closely related substances, differing only in a slight variation in the arrangement of their atoms in space, a given enzyme is neutral, while acting readily upon the other. A slight change in the latter renders it, too, insusceptible. The proof of this led Fischer to the conclusion that enzymes act upon their substrates through exact spacial approximations, his famous key and lock analogy.

So much to show the nature of Fischer's work, to indicate the type of mind of the man. It was by no means the only, or even the largest, problem this brilliant and versatile chemist undertook. In another class of compounds, the proteins, was a chaos which seemed of inconceivable complexity until Fischer pointed the way through. Proteins are peculiarly biological; without them there would be no life, and differences in protein are probably ultimately responsible for differences in living forms, inasmuch as protein must carry the hereditary characteristics. They are found in greatest bulk in the animal kingdom, though not confined to it, and include such well known substances as egg white, the casein of milk, and flesh of all forms. In the analysis

of these proteins, involving breaking them down and identifying the products, Fischer played a great part, his greatest contribution being the development of a method which enabled others to expedite the solution of the colossal problem. Difficult as the analysis has been Fischer was at the time of his death in a fair way of accomplishing the reverse, the synthesis or reconstruction of a highly complex protein from its split products, a tremendous conquest of the world of the unknown.

One more glimpse and we shall leave the subject of Fischer's work and place our label on the man. Combined with certain proteins in that mysterious portion of the living cell which apparently governs the latter in its chief actions, the nucleus, is a substance called nucleic acid, characterized in part by the presence of certain bases, the purines. The familiar uric acid is a member of this group. Fischer, working on uric acid and the related substance, caffeine, synthesized both, and not only them but all other members of the group. It was a work of prime importance chemically and brings up for our consideration a special point. Fischer commercialized his artificial synthesis of caffeine, that substance both praised and reviled, and made many thousands of dollars. The emphasis placed upon this associated his name in many minds, especially those of medical men, with the substance caffeine, and in superficial biographical treatment it has been customary to list its synthesis along with his other great accomplishments and on the same plane. As a matter of fact it must be thought of as purely by the way. Beyond question what really counted in Fischer's mind was not the synthesis of this isolated substance but the unshrouding of the relations of that whole group of compounds of which caffeine was but an ordinary member, making the previously confused subject as clear as the light of day.

Fischer was an aristocrat in scientific work if there ever was one. No man can explain this devotion to his science. We can perhaps say that the ability created the desire, but most men work for rewards, and what was Fischer's? Fischer made money, but no sum of money could be adequate pay for the superhuman accomplishments of the man, nor did he make a tithe of what he might have made had he tried. He saw himself in the front rank of scientific men, but the contemplation of that spectacle must have palled upon his cold judgment. The only thing that remains is the thing itself. Pushing back the horizon of knowledge was its own reward.

Scores of other pairs might be cited to illustrate the grouping over and over again. A fascinating book might be written by plagiarizing a method from Plutarch and setting down side by side the biographies of great scientific men, laboring on similar lines, devoted to different points of view. We could thus pair off contemporaries, and going

closer, warm friends, and sometimes master and pupil. What could be more characteristic than the distinction in the attitude of Darwin and his brilliant spokesman, Huxley? On the one hand, we have the quiet naturalist of Down turning over in his mind for twenty years observations made on the voyage of the Beagle, and the culmination of that reflection in the scientific torpedo of the nineteenth century, *The Origin of Species*. And on the other, stands the rugged Huxley, self-appointed champion for truth, the most lucid scientific writer of the day, meeting all comers in defense of the theory of evolution, nay, going much further, carrying war into the enemy's country, extending his campaign beyond the field of biology and evolution and, through the medium of the public press, hammering into public consciousness the scientific method in general. In Darwin's own words in his autobiography we have this; "I think I can say with truth, though I cared in the highest degree for the approbation of such men as Lyell and Hooker, who were my friends, I did not care much about the general public. I do not mean to say that a favorable review or a large sale of my books did not please me greatly, but the pleasure was a fleeting one, and I am sure that I have never turned one inch out of my way to gain fame." He must have been regardless of public approbation who spent eight weary years on the Cirripedia, describing all known living species, and publishing two quartos on the extinct species, and who passed from the *Origin of Species* to a book on the "Fertilization of Orchids." But Huxley was different. Brilliant investigator that he was himself in the field of pure science, he cared more for something else. The education of the masses was the supreme thing. He fought to give the people the truth at a time when they did not want it, and how he came up from the bottom is appreciated by few of the succeeding generation who take his victory for granted.

We might confuse the issue a little and pair off Sir Humphry Davy and his pupil, the immortal Faraday, perhaps the most lovable man science has ever produced, and, if we except Pasteur, the favorite of all biographers, Faraday, who never knew what it was to have a decent income, and presented to the world a set of scientific experiments the cash value of which to mankind was estimated by Brailsford Robertson some years ago at seventy five billions of dollars. For to Faraday we owe the motor and the dynamo.

Which was Faraday? The aristocrat surely, we say when we recall that he repeatedly dropped his investigations when they neared the point of marketable value. But he never lost sight of the latter. "I had rather," he said, "been desirous of discovering new facts and new relations dependent on magneto-electric induction than of exalting the force of those already obtained, being assured that the latter would find their full development hereafter." And when asked of the possible

utility of some of his discoveries he was wont to reply in the words of Franklin, "What is the use of a baby?" And it is on record too that in dispute over the priority of certain scientific discoveries he stood resolutely on his rights on more than one occasion.

And Davy, baronet and pet of society, aristocrat in the common acceptance of the term—which was he? "A philosopher" he was in the old terminology, and the "philosophers" were mostly aristocrats in science. His fame rests chiefly on his discoveries in electro-chemistry, but we can not forget the ten years of lecturing in the cause of agriculture and his own experiments for its promotion. And there is the deliberate and successful attempt to produce a safety lamp for miners, followed by the presentation to him of a set of plate worth two thousand pounds by the coal owners of the Tyne and Wear in testimony of their appreciation of the benefit thus conferred. And we know that his last great scientific endeavor was an effort based on exact deduction from the laws of electro-chemistry to protect the copper sheating of vessels from the corrosive action of sea water. Vain and selfish he may have been, but he must have had the interest of the people at heart, and so we can place that to his credit against his oft cited jealousy and ill treatment of his rising, more brilliant pupil, Faraday.

Probably the greatest scientific democrat in history is Louis Pasteur. No more patent testimony of public affection for a scientific man has ever been given than that expressed in the recent French plebiscite on the greatest man of the nation, which went easily to Pasteur with Sadi-Carnot and the great Corsican running second and third. And yet, as we have defined the term, Pasteur began his researches as one of the aristocrats; it was his brilliant success in a field of pure science which laid the foundation and directed the way for his later, more famous, investigations on the etiology of disease. It was in the course of his highly technical work on the optical rotation of the salts of tartaric acid that he discovered the fact that of its isomers one form is destroyed by fermentation, and the other not. These isomers, like the sugars described above, differ from each other simply in the arrangement of the same atoms in space. He thus anticipated by many years Fischer's classic work on enzymes. But in their application of the facts thus secured is strikingly emphasized the wide divergence in the point of view of the two men. Fischer's work on ferments constantly led back to the theory of sugar structure. With Pasteur's successful solution of his problem he turned the page and with new interest in fermenting bacteria entered upon that lifelong endeavor to thwart the action of pernicious microorganisms that has made him one of the greatest benefactors of mankind.

How he passed first to the relief of stricken industry, adopting means, based on his discoveries in bacteriology, for the cure of

"diseases" of beer and wine, and of the fatal silkworm disease, and thence to the extermination of those plagues, fowl cholera and anthrax, which were wiping out the flocks and herds of France, saving to the country in a matter of months the equivalent of the Franco-Prussian war debt, and thence to preventive vaccination in man, has been the theme of a score of biographies and many an inspiring address. The life of this non-medical man is held up as a shining example by the most intolerant of professions for the guidance of its students and future fellow physicians.

Commenting on aims in scientific research the famous British chemist, Roscoe, chose a text from Pasteur to bear him out in his contention that although it would be foolish and short-sighted to decry the pursuit of any form of scientific study because it was as yet far removed from practical application to the wants of man, yet discoveries which tend to diminish the ills that flesh of man or beast is heir to, deservedly create a more general interest than those having no direct bearing on the welfare of the race. In the French hero's simple words, "There is no greater charm than to make new discoveries, but the pleasure of the investigator is more than doubled when he sees they find direct application in practical life."

The same year that Pasteur graduated from the Ecole Normale in Paris a young German physician, but a few months older, read, before the physical society of Berlin, one of the two epoch-making scientific contributions of the century, "Ueber die Erhaltung der Kraft" rivaled only by the *Origin of Species* in its effect upon thought. The notion of the Conservation of Energy may not have originated in the precocious intellect of Helmholtz, for Newton, DesCartes, Leibnitz, Lavoisier, Mayer, Colding and Joule had more than touched it, but it surely crystallized there. It remained for a physician to desert his chosen field for a theoretical one and establish the fact that energy, although it may be transformed in kind, is indestructible, and the total quantity in the universe is constant. The physiologist was also a mathematician of the first order.

Perhaps the most frequently cited achievement of Helmholtz is his invention of the ophthalmoscope, that familiar combination of mirror and lenses used in examination of the interior of the eye. How did he come to devise it? As a matter of fact it took its origin in a simple desire to exhibit a physiological phenomenon to his students. It had long been known that light could be reflected from the back of an animal's eye, but no one had yet been able to put his own eye in such a position as to have reflected directly back to it light from the illuminated eye in concentrated form. Helmholtz, by the proper disposition of mirrors, involving a knowledge of optics and the anatomy of the eye, accomplished this for the first time, and, eight days after

he conceived the desire to show it to his students, he was the first to see the living human retina.

While following the invention of the ophthalmoscope, Helmholtz was occupied for some years with physiological optics, it can not be said that it "set" his career as it might have done with another man, as for instance his invention of the stethoscope did with Laennec. The latter putting a paper cylinder to a consumptive girl's chest discovered that through it he could hear the sounds of moisture vastly better than by direct application of the ear. Here was a method, and the young clinician devoted the rest of his own all too short consumptive's life to diseases of the chest. Helmholtz turned the so called practical application of his ophthalmoscope over to the clinicians and busied himself with the discovery of new facts and explanation of old, quite regardless of their direct application to the ills of man. The physics was the more attractive field than the pathology, and to the end of his life he gravitated more and more from the latter toward the former, until the boy of twenty-eight, who started out as a professor of physiology and pathology at Königsberg, became at fifty the dominating figure of the time in physical science as professor of physics in Berlin. While he was one of the masters of medicine, his contributions were toward theory and not practise, his name being associated in the science chiefly with the explanation of vision and hearing and the theory of color and tone. That immeasurable practical use of his discoveries is being made goes without saying. But it is indeed a fortunate thing for mankind that the superb intellect of Helmholtz was not turned into the superficial channels of practical usage but was left free to explore the unknown depths below.

In later years his trend of thought took him more into the field of dynamics and electro-dynamics, bringing him back to the direct course on which he started forty years before with his immortal essay on the Conservation of Energy. His intellectual goal was never a democratic one. He felt it his duty as a disciple of science to ascertain truth for its own sake, and no man more strongly decried the pursuit of science merely for the practical results. His clear exposition of the principles of the transformation of energy furnishes the index of his character, and, whatever the practical applications of the principles thus enunciated may be, we can not avoid the impression of something essentially aristocratic in a young man of twenty-six killing for all time with one blow, the idea of a machine for perpetual motion.

Obviously the points of view of Pasteur and Helmholtz were fairly far apart. This does not mean that the one was consistently democratic and the other at all times aristocratic in the sense defined above, throughout their long lives of scientific research. Many times the rôles were reversed. Helmholtz contributed a practical remedy for

nasal catarrh, and Pasteur evolved a theory of the essential nature of fermentation. But the dominant note of each was as given above. Pasteur burnt himself out in the effort to bring relief to a mankind patently suffering before his eyes. Helmholtz consumed his candle more slowly, pursuing the even tenor of his way without committing himself on his sympathies.

Undoubtedly the distinction is but vaguely general, and perspective may be needed to bring it out. Undoubtedly there are no pure types. Scientific men, goaded by poverty or other considerations, may and often do change the direction of their researches, like the rubber synthesist of "Marriage." The war brought out many a quiet man from purely academic life to construct a poison gas, or detect a submarine, or relieve a starving people. But most of these men have gone back. The will of the people was temporarily irresistible. But characters were not changed much. A certain group are altruistic from the start, and do not need to explain their motives. The other group are exclusive and do not try to explain theirs, except to each other. They isolate their intellectual lives to a certain extent, in all the pride of an aristocracy based on achievement rather than accident. The achievement is the increase of knowledge, and that is both goal and prize.

# THE CONDUCTION OF RESEARCH

By F. H. NORTON

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RESEARCH may be defined as the process of intentionally looking for something new; the value of some physical constant, a new material, or a method of performing some operation. According to its purpose, research can logically be divided into two classes; the first is pure research which includes the study of the properties of natural objects, such as the determination of atomic weights, the distance of a star, or the development of plants and animals, while the second may be termed industrial research which includes the development of a material having commercially useful properties, or the refinement of a process so as to increase its output or decrease its cost. Such a division can not of course be strictly held to, for the two classes intermingle at times, as there are a great many cases where an investigation, starting as pure research, later developed industrial possibilities. In fact there are very few pieces of pure research that do not sooner or later offer valuable contributions to the industries.

A very large portion of the modern development in science is due to systematic research. While it may be true that some highly interesting facts are stumbled upon in the realm of science, it is the thorough, painstaking investigations conducted at our government, university and industrial laboratories that in the last twenty years have given us such a remarkable insight into the structure of matter and the universe.

Engineering, which has in the past, clung to cut and try methods, is beginning to realize the value of trained men and well-equipped laboratories in solving its problems. Perhaps no division of engineering has advanced as rapidly in this respect as aeronautics, comparatively a very new subject, and yet, because of its exacting demands, has caused the development of methods so advanced that they may well be borrowed by the older branches. Commercial enterprises of all types are developing research by organizing their own laboratories or by financing the research laboratories of the universities. Many concerns are devoting five to ten per cent. of their profits to research, and some have laboratories employing scores of trained men. The industrial research laboratory is past the experimental stage and is recognized as a sound financial asset.

Research may also be divided according as it is definite or indefinite, that is, whether or not it is known beforehand what is being

searched for. An example of the first class would be the determination of the melting point of an element, while the development of a machine for making bricks belongs to the second. The first class is the more straightforward, so that the program can be planned in advance with the possibility of only minor changes, and the final result will be uncertain only in its degree of accuracy. A research of the second class is more difficult, as it is a process of trial and error, the most hopeful ideas being tried out until by the knowledge gained from previous failures, a successful result is obtained. But, unfortunately, in this class of research a successful result can not be guaranteed in advance, and consolation must sometimes be obtained by the knowledge that a well tried failure is a step ahead.

Perhaps the first thing to do when commencing a piece of research is to make a thorough study of the available literature on the subject; first, to get new ideas, and, second, to prevent the repetition of methods that have been found unworkable. On the contrary, there are certain types of original thinkers who claim that they do not wish to know what any one else has done on the subject, as they believe that this knowledge will hamper the freedom and originality of their work. This manner of working certainly leads to much needless duplication and is not to be recommended except for very special cases. It is unfortunately true that many statements appearing in print are in error, due to poor experimentation, wrong conclusions and arithmetical blunders, and it is strange how long these erroneous statements are accepted. There are many instances where results of this type have been quoted several times by other authors before their absurdity has been made evident. It is, therefore, advisable to take all information on trial, and to take no one's statements for granted without giving them a thorough examination.

After all the important information has been reviewed and carefully analyzed, a plan, or several alternative plans, can be laid out for the most promising manner of attacking the given problem. Although the plan will undoubtedly have to be revised or even completely changed during the progress of the investigation, it is much better to have a definite program to start with than to work along in a haphazard manner, principally because of the training gained by the experimenters in an orderly procedure. It is foresight and anticipation of difficulties at this time that saves endless complications later on, and the value of a research director depends on his ability to keep his men from getting into trouble, or, if this is impossible, to get them out as quickly as possible. The more experience one has had in research the more clearly can he visualize the subsequent procedure, and the final results.

If the research being undertaken is of a simple character the only

apparatus needed will be of a standard type that can be readily obtained, but in most cases it will be necessary to build special apparatus. There are two ways to design research apparatus; the first, is to lay out an expensive and elaborate design on the drafting board without experimental trial of any of its parts, the second, is to carry on the experimental work as far as possible with inexpensive, temporary means, until the soundness of the method is assured, and then designing the apparatus from the knowledge gained in this manner. The first method may work out satisfactorily but more often it does not, and an expensive instrument or machine must be greatly altered or scrapped. The second is not as rapid nor as convenient, but it is safe and sure and should certainly be used unless there is a large fund of past experience to draw from in regard to that particular subject.

After the general type of apparatus is decided upon, there is its detailed construction to consider. If it is to be used for only a short time, there is no need to go into elaborate design, or neat finish; the main points to think of are cheapness and quickness of construction and ample opportunities for making alterations. It is always best to rebuild a standard type of machine if possible, and it is surprising how a little ingenuity will make a working device out of the most common parts. It is natural that an experimenter should desire to have an original and neat piece of apparatus, but first it should be determined whether the increased efficiency in operation will warrant the necessary expense. However, it is often the case that a little extra expense if applied efficiently to finishing a piece of apparatus will effect a saving in the end by preserving it from the attacks of moisture or chemical fumes. At the same time a well finished job, even though it will not actually give more accurate results, takes it out of the home made class, and gives to the report illustrated by its photographs, a workmanlike tone that is of the greatest value in raising the standard of that particular investigation. Although an expensive and well finished instrument does not assure excellence in the experimental work, yet the person reading the report is unconsciously affected by the appearance of the apparatus, crude apparatus being associated with uncertain results. On the other hand, there is undoubtedly a tremendous amount of money spent each year on unnecessarily elaborate machines and instruments, but the waste is usually due more to the fact that the complete design is unworkable, than that the elaboration is unnecessary.

There are other types of research that extend indefinitely into the future, using principally the same apparatus in all the tests. Examples of this are tests of material strength, or model testing is towing basins or wind tunnels. In these cases the conditions are far different from the isolated investigation, and it is simply a question of how far it will be advisable to go, in using automatic and recording devices to reduce

the personnel required in the testing. For example let us consider a wind tunnel balance for measuring three forces and three moments on an airplane model. Ordinarily two men operate the balance, reading two of the required quantities at a time. If it were a question of time, the balance might be arranged to use six observers who would read the six quantities simultaneously, but this would increase the operating expenses. The next improvement would be an automatic balancing arrangement so that one observer could record all six quantities, but this would require a much more expensive balance. If it was desired to reduce the labor still further, the work of several draftsmen could be dispensed with by making the balance record and plot all six quantities, a process that is actually quite simple to do, but which would require still more additions to the balance. The elaborateness of a piece of apparatus, then, will depend largely on how much it will be used, for there would obviously be no use in building an expensive and labor saving machine for a few experiments, nor would it be economical to use a cheap and inconvenient machine for an extended series of investigations.

Another particular that should be kept in mind when designing apparatus is a construction that will make the calculation of results simple and convenient. It very often happens that a small change in an otherwise excellent instrument will save days, and perhaps weeks of computation. As an example of the importance of this, it may be stated that in a certain investigation on an airplane in free flight, the data was collected in less than ten hours of flying, but required the time of three men for four months to work up the final results, and there are other experiments where the ratio is even greater than this.

The economy of having a well equipped research laboratory is soon demonstrated by the saving in time and expense in setting up for an experiment, and the older a laboratory is the more apparatus there is accumulated from which to select. In any experimental work a large junk pile is invaluable, and until this is collected, the true experimenter cannot work efficiently. The laboratory should have a shop of its own, or immediate access to one, equipped to do the class of work desired, as the ability to easily get small parts constructed or alterations made is of the greatest importance in the efficient conduction of research. Nearly all classes of research require in some way the application of photographic methods, so that a dark room is a valuable, sometimes a necessary adjunct to the laboratory.

With the construction of the apparatus completed, we come to the actual carrying out of the research. The first thing to do is to set up and try out the apparatus and to determine with what accuracy the results may be depended upon. It is good policy, especially in extended experiments, to take plenty of time in the beginning to get all

parts in reliable operation, or much time and accuracy will be lost later on by breakdowns. Often times a slipshod arrangement is allowed to stand with the hope that it will hold together during the test, but nothing can be more discouraging than to find at the completion of a long run that some little thing had gone wrong and rendered the results useless.

Perhaps it would not be out of place at this time to touch on the subject of precision of measurements. It is believed that a lack of understanding of this subject leads to a large amount of extra work being done, and accuracy sacrificed, in many branches of research. There is obviously no use in obtaining data, or of computing results, to a much higher degree of accuracy than the least precise component, and it is the neglect to find the precision measure of this component that leads to much needless computation. On the other hand, it sometimes happens that if the least precise component were recognized it could be obtained with greater precision, thus increasing the accuracy of the whole experiment. It often happens, too, that certain factors having only a slight influence on the results, are either recorded when their effect is smaller than the errors in the other factors, or they are neglected when that effect is larger than the errors introduced from all other sources. For this reason, every condition that can in any way effect the final results, should be carefully analyzed, not only to determine whether it can safely be neglected, but to find out how closely it need be measured if it can not be neglected.

The successful carrying out of an experiment requires the constant checking up of the data obtained, in order to detect an error before it has invalidated a long series of runs. It is only by constant vigilance that errors can be excluded from the work, and it is the ability to detect irregularites that will cause future errors, or to detect the errors themselves before they can cause trouble, that distinguishes the true experimenter. For example, let us consider a certain test to determine the effect of varying the aspect ratio of a model airplane wing in the wind tunnel. The procedure consisted in making a test, then cutting off a small length of the wing, and repeating the test, continuing the process until the span of the wing was reduced to a small amount. It would be extremely unwise in an investigation of this kind to collect all of the values without working up the data and constantly comparing it with the preceeding results each time before cutting off the wing. Otherwise, it might be found that one or more of the runs did not agree with the rest, due to a lack of alignment or to some other type of error that is apt to creep into any experimentation. After the wing had been cut down, however, it would be too late to make a check run and the whole test would be invalidated or at least made to appear of doubtful value.

Before beginning an experiment care should be taken that everything is functioning properly, and that all disturbing factors have been taken into account. Never take a chance, as every research man should realize that not only his reputation, but that of the organization with which he is connected, is endangered by his mistakes, and he should under no conditions allow results to come from him, unless he is very sure of their correctness.

It might seem unnecessary to bring up the subject of honesty in research, for there would seem to be very little reason to give results deliberately in error. It has often happened, however, that an experimenter has shaded the values of his readings to make them come closer to what he supposed was the true value, but he often finds later, that he has gone in the wrong direction, and readings taken in this way will give neither the true mean nor the probable error. Sometimes this squeezing of results up or down is done quite unconsciously, and for this reason when it is desired to make a check run, the results of the first run should never be in sight, or it will not be a true check, even though the recorder is not in any way intentionally dishonest.

When it is desired to determine the accuracy of certain data, or to be assured that it lies within the permissible limits of accuracy, it is customary to make two or more runs under identical conditions, the difference between the values obtained in each being an indication of the accuracy that may be expected. This does not, however, tell the whole story, as it does not take into account those errors in the design or setting up of the apparatus, or the individualities of the experimenters. For this reason, it is always well when making a check run to reset the apparatus or, better, to use a different piece of apparatus and different observers, in which case the results may be considered to give a true indication of the probable error from all sources. It often happens that a certain set of facts are not obtainable in a direct or simple manner, nor is the best method that can be devised entirely satisfactory. In such cases it is always best to obtain the results in several different ways, and, although none of them may be satisfactory, yet, if the several results show an agreement, it may be concluded with certainty that they are correct. Even in the more straightforward investigations, wherever possible, the results should be checked up by an alternative method, as this is an excellent way to make others have confidence in the data.

Next to accuracy, the most important consideration in research is efficiency, that is, the obtaining of the largest amount of results for the least expenditure of time and money. Efficiency can only be attained by the careful laying out of the work, the careful determination of what is necessary to do to get the required accuracy, and, most important, to have a smooth running organization. The laying out of the

work consists in ordering materials, having apparatus designed and constructed and deciding on the methods to be used. The laying out of the work in a manner to promote efficiency depends largely on the foresight and experience of the experimenter or research director. For this reason, it is false economy to employ low grade or inexperienced research men, as the saving in the pay roll is more than offset by the decreased value of the results and the increased cost of the investigation.

As time is usually as important an item in efficiency as cost, the ordering of necessary supplies should be accomplished as early as possible, and when big delays are impossible to avoid because of the lack of some material, it may become necessary to alter the experimental methods in order to be able to proceed within a reasonable time. It is very costly to have everything set for performing an experiment, and then to find that some small but vital thing has been forgotten which will take weeks to procure. All phases of the preliminary work should be constantly checked over and the most delaying items followed up vigorously.

The previous discussion of efficiency has been confined to a single piece of research, but usually a number of investigations are going on together in one laboratory, perhaps a separate group of men working on each research, or a group alternating between several types of work as conditions permit. In this case it is the duty of the research director to arrange the work not only so that every one will be kept busy, but so that each man will be working to the best advantage. Every one is more or less of a specialist, and it is of considerable advantage to have each person kept as far as possible on one type of work. This can not, of course, always be done, but by carefully laying out the work ahead in this respect, it will be possible to have the men working in their most efficient positions a large part of the time. The same thing applies to pieces of standard apparatus such as balances, testing machines, etc., so that the work should be planned to use the equipment as efficiently as possible.

In regard to the selection and training of the research personnel, it will be best to first discuss the types of men available, exclusive of their particular training. In the first place men may be divided rather sharply into two classes, the first we will call practical, and the second, theoretical. The first class have mainly gained their knowledge from experience, are mechanically inclined and know how to use their hands, while the second class have obtained their knowledge almost exclusively from books and have very little commonsense in regard to mechanical matters. For example, one of the theoretical class may be able to make a complex computation of the stresses in a certain small bolt, and yet when screwing in the same bolt he will calmly twist its head off, simply because he has no mechanical sense. The latter are a type that

are of great value in mathematical work, computations and the writing of reports, but when it is attempted to use them on purely experimental work, their efficiency is greatly reduced. It is far easier to teach one of the practical class to work efficiently on theoretical problems, than is to teach one of the theoretical class to work efficiently with their hands. It is quite necessary to have experimenters who can be trusted with delicate apparatus without having constantly to fear for its safety.

Some men are naturally hustlers and possess initiative enough to make themselves of value with very little supervision, and some go so far in this direction as to require constant restraint to keep them from getting beyond their depth, but this quality is on the whole a good one, and should be directed rather than discouraged. On the other hand, there are men who will take no responsibility and need constant pushing to keep them working efficiently, but wherever possible it is better to lead than to force. If a person is really interested in his work, and by interested, I mean the ability to derive pleasure from thinking of the problems evolved outside as well as inside of working hours, he requires no pushing and very little directing, so that the whole problem of successful administration lies in getting the staff interested in the problem on which they are working.

As far as possible it is best to give to each man a definite job, and make him responsible for it; giving him the credit when it is successfully completed. This stimulates interest and originality, and is much better than a constant supervision down to the smallest details, a method that is likely to produce ill feeling and retard the development of the experimenter. Of course, an inexperienced man can not be efficiently put on to a new subject without considerable supervision, but this supervision should be instructive rather than destructive, and as soon as he shows himself capable of handling the work he should be left to carry on alone. When, as is often the case, a number of men are working on one problem each one should be encouraged to acquaint themselves with the work of the others in order to obtain a more general view point.

It often happens in research work that certain portions of it, such as computations, are exceedingly monotonous and it would certainly be an injustice to give all of this portion to one man, so that it is always best to distribute this kind of work among the investigators, unless, of course, some of the men are especially hired for this, and have no experience fitting them for other work. There are some types of research that can be most efficiently carried out by a single person working exclusively on that job, but in the majority of cases it is better to concentrate a number of men on the problem, not only to finish it up and get it out of the way quickly, but because a man is apt to get into a rut when working alone. There are some experiments that require a high

degree of manipulative skill and to these should be assigned the type of man who is naturally handy with his hands. Conversely there are problems of a mathematical nature that would be exceedingly irksome to the preceding type, whereas the theoretical type could handle them efficiently. It is the problem, then, of the research director to so arrange the work that each man of his staff will be working to the greatest advantage.

Undoubtedly the most important quality that any experimenter can have is persistence. There are many problems that require months and perhaps years of hard, discouraging work before the first ray of success can be discerned, and in such cases it requires the utmost faith in the ultimate result to enable one to keep up his interest. It is only by the careful, systematic elimination of each obstacle as it comes up and the direction of the work continually into new and more promising channels that will make successful what to the less determined experimenter would be a failure.

Perhaps it will not be out of place to say a few words about the presentation of results. In the first place, the data should be given, wherever possible, in graphical form. There are some instances where the accuracy of the results is greater than can be represented by a plot, in which case the data must be also given in tabular form, but in most cases a curve is sufficient and the tables may be omitted. When plotting curves, no points should be used except those directly computed from the experimental results, as the practice of some very reputable laboratories of taking points from a faired curve as the basis of plotting is very misleading as to the regularity of the results. On the other hand, no experimental curve should be shown without including the actual points, otherwise the results can not help but be regarded with suspicion. It is always better to present a few well checked results than a multitude of irregular ones.

One of the secrets of experimentation is to know when to stop, for it is a natural tendency to carry the work further than the value of the additional results will warrant, and it is inefficient to allow a nearly finished piece of research to drag along. There is of course, no definite point where a piece of work can be considered finished, and often times one feels that he is in a position to efficiently commence the work only when the allotted time or money is exhausted. For this reason one of the most important functions of an investigation should be the paving of the way for more extended work. Therefore every report should contain an account of the difficulties encountered, and most important the recommendations of the experimenter for the conduction of further research, for every difficulty, and every failure, should be made of value by preventing others from encountering the same obstacles.

It is urged that every investigation that produces results of interest

or value be published or in some way be made available to those who are interested. It now happens, especially in the industrial laboratories, that much work is done which is never known outside, and there is generally no reason why the results should not be published after the particular organization has received its benefit. Because of this practice a great deal of money is spent in duplicating work that has been already accomplished, and, while a certain amount of duplication is valuable as a check, it is in general very uneconomical. In the same way, it sometimes happens that similar investigations are undertaken simultaneously, and, although their results are later published, it means an unnecessary duplication. In this particular it would be of great value to have a research clearing house where all the work in preparation could be gathered together for general information.

It may be stated in conclusion that the carrying out of a piece of research will comprise in general the following procedure: First, the similar work of others is studied, especially their difficulties and failures, and from this information a plan of operation is laid out. And if there is any doubt, and there usually is, as to the practicability of the proposed methods, preliminary experiments should be conducted, from which data are obtained for use in designing apparatus and for more completely planning the subsequent procedure. In carrying out the actual work, the first consideration should be accuracy and the second, efficiency, both depending on suitable equipment and on an interested and well-organized staff. Lastly, the experimenter must organize his results, and deduce from them conclusions that will be of value in joining them with similar work and in advancing the theory and practice of the subject. Briefly, successful conduction of research depends on the foresight and vision of the experimenter in laying out the work, his accuracy, persistence and manipulative skill in carrying it out, and, lastly, his analytical ability in deducing conclusions from the results.

# A BIOLOGICAL EXAMINATION OF LAKE GEORGE, N. Y.

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DURING the summer of 1920, the New York State Conservation Commission maintained a field laboratory on Juanita Island in Lake George. The writer was placed in charge. Dr. Chancey Juday of the University of Wisconsin, was at the laboratory in August studying temperatures, plankton, and dissolved gases of the lake at different depths. Dr. Emmeline Moore, of the Commission, was detailed to assist in the work of studying the food of fishes and of tracing it back to its sources in the lake vegetation of the shores and of the plankton. State Fish Culturist, Mr. John W. Titcomb, of the Commission, was also present during August studying fishes. Messrs. Charles K. Sibley, of Kingston, N. Y., and William R. Needham, of Ithaca, were regular assistants at the laboratory during the whole of the season. Visiting naturalists who participated to some extent in the work of the laboratory at times during the summer were Mr. S. C. Bishop, New York State Zoologist, of Albany; Professor C. R. Crosby and Dr. M. D. Leonard, entomologists of Cornell University; Miss Sue J. Reid, secretary of the Chicago Nature Study Society, and Miss Jay R. Traver Supervisor of Nature Study at Wilmington, Delaware. The object of this laboratory was to determine conditions in the lake affecting the life of food and game fishes. A report has been made to the commission on fish cultural matters. And at its completion it has seemed to the writer that a number of observations made in the course of this work that are of a more general scientific sort might be helpful to other field naturalists and ought to be placed on record: hence, this paper.

The water of Lake George is "soft"; and the dominant plants and lesser animals are quite different from those of the lakes in Central New York. Doubtless the most abundant plant in the lake—the one that bulks largest—is the stonewort *Nitella opaca*. It occurs at depths between 18 and 45 feet and covers scores if not hundreds of acres of the lake bed between these depths. It forms great meadow-like beds of erect or recumbent, soft, translucent bright green stems often three or four feet long. These beds (called "grass" by the local fishermen) furnish shelter and support for a large population of sessile algae; for case-inhabiting insects, such as caddis worms and midge

larvae; for free-living animals such as mayflies, snails and scuds, and for sessile hydras and rotifers. About these beds most still fishing is done by the initiated.

Other stoneworts observed by us were all found in quite shoal water—less than 10 feet—and the most interesting of them was the extremely delicate and remarkably pretty *Nitella batrachosperma* that occurred at 5 feet, on beds of yellow ripple-marked sand, and swayed in passing waves with all the aspect of a bright green frog-spittle This occurred sparingly but was found by us in places as wide apart as the head of Northwest Bay, the south shore of Juanita Island and the outlet channel below Baldwin. Two species of Chara also were found in the shoals.

In the lake bed below the level of the Nitella zone there grow two peculiar and characteristic green algae. One is a "Siphon alga" *Dichotomosiphon* that at 40 to 50 feet in depth is so abundant as to be a nuisance to the lake trout fishermen: the lead bob of their trolling apparatus gathers the tangled and matted threads of this plant about it and brings them to the surface in slimy dripping handfuls. This is what the fishermen know as "moss." Dr. Moore is publishing elsewhere an account of this species. The other alga, a species of *Cladophora* invades the depths where hardly any light penetrates. Its minute branching sprays usually about a fourth of an inch long and of very bright green color may be found sparingly, lying amid the bottom sediment at almost any depth in the lake below the shoals.

The most abundant filamentous alga of the shore is a species of Tolypothrix which fairly covers submerged stones and timbers with its little tufts of brownish-green swaying threads, always and everywhere interspersed with masses of gelatinous palmelloid forms. Among the coarser waterweeds hornwort and elodea are quite scarce and the fine tall-growing *Potamogeton praelongus* is conspicuous, forming some of the most beautiful weed beds of the lake. These weed beds occur mainly at depths of 10 to 15 feet. They shelter forms similar to those of the Nitella beds and many additional, including two interesting waterfleas, *Acantholeberis curvirostris* and *Eurycercus lamellatus*. The latter is abundant enough and large enough to form an important item in the diet of some of the adult game fishes of the lake.

In the plankton of the lake, diatoms of four genera were probably the most important food gatherers. Asterionella, Cylotella, Tabellaria and Fragillaria. Other algae less constant but occasionally abundant were Anabaena, Aphanizomenon, Aphanocapsa, Botryococcus, and Staurastrum. Among the cladocerans, which all season through were far more abundant than copepods, Bosmina was rather common throughout the season, Polyphemus became abundant for a time in

July, Holopedium attained an extraordinary dominance the latter part of July and two species of Daphnia replaced it in late August. On July 14th a net of No. 6 silk drawn for a few minutes in the lake gathered a solid pailful of Holopedium in a nearly pure culture. A few Leptodoras and a few Daphnias were present besides.

We found some good collecting grounds for aquatic plants and animals; and it may benefit some future naturalist who is visiting Lake George for the first time if I mention a few of them. First of all, Juanita Island itself, our headquarters, has most interesting shores. On the west the rocks rise vertically out of the water; on the north they run down in gently sloping serried low ridges of solid rock, smooth and bare as far as the breaker line; on the northeast is a broad smooth sandy beach in a sheltered bay (here was our bathing beach); on the south is a shore line of broken rocks and at the east this merges into a narrow beach of ripple-marked sand. Eastward of the Island is a deep current-swept channel, and northward is a more sheltered cross channel in which the "grass" and "moss" of the fishermen are found. There are scattering growths of Potamogeton, Ceratophyllum and Heterantheria below the breaker line (which occurs here at about 5 feet below the surface) and just above it grow Valisneria, Eriocaluon, and Lobelia.

There is a very interesting admixture of small plants growing in the rippled sand about the dock and at the edges of the beach within the bay. The most abundant plant present is one of the least conspicuous, *Myriophyllum tenellum*, a true sand-binder of the shore, whose tufted, slender interlaced stems lie buried in the sand, and whose many leafless red branches rise erect but an inch or so above the surface, and, draped with tufts of filamentous algae, are most inconspicuous. More in evidence is the little creeping *Elatine americana*, that formed close-growing patches the size of a silver dollar on the surface of the sand, and that is fairly covered in August with minute blossoms. Intermixed with it, and likewise persistently blossoming is the curious little cruciferous quillwort, *Subularia aquatilis*, which grows erect to a height of perhaps an inch above the surface of the sand. Another pygmy component of this inch-high vegetation is the slender creeping spearwort, *Ranunculus flammula filiformis* which here spreads by stolons about an inch long in single lines of progression over the sand. Another is an undetermined closely tufted spike-rush, whose roots bear numerous slender little brownish tubers.

There are also scattering plants of taller stature here; bushy little sprays of *Nais flexilis*, pinnate sprays of *Potamogeton perfoliatus* and *P. heterophyllus*, the latter having when grown two or three oblong leaves that reach the surface. There are small tufts of eelgrass, *Valisneria spiralis*, in the more exposed places on tufts of two species

of quillwort, *Isoetes* and *I. tuckermann macrocarpa* in the edges: there are also similar tufts of *Sagittaria graminea* with only the stubby basal leaves developed. But where there is sufficient shelter to avoid burial of its tuft-forming leaves by the sand, the pipe wort, *Eriocaulon articulatum*, comes in and forms a complete ground cover. About the edges of the mats of pipe wort are always a few water lobelias, *Lobelia dortmanni*, and the slender white stems of the former and the purple stems of the latter shoot upward together to the surface at flowering time in August.

At the west and about the charming little Boquet Island large fresh-water mussels abound wherever there is enough soil accumulated among the rocks to give them footing; and a little farther away to the southward the clay bottom about the Ranger Pool is fairly covered with a little translucent white clam, *Sphaerium tenue*, having siphons of a charming rose-pink color.

Shelving Rock Bay on the eastern shore of the lake a mile south of Pearl Point is a very fine collecting ground, having in close proximity a large variety of aquatic situations. A charming mountain brook, spring fed and full of trout, enters the head of the bay. A storm-wave reef across the head encloses a marsh of a few acres mostly overgrown with sweet-bay, but having small areas of open water, where pike and bullheads spawn. So great is the abundance and variety of marsh life found here that I can hardly specify particulars. I will only mention the abundance of desmids among the algae, and of the clusters of the bryozoan, *Lophophus crystallinus*, about the bases of emergent sweet-bay stems.

The bay itself has a broad quietly-sloping wave-swept sandy beach, one of the finest on the lake for a bathing beach. Further out toward Iroquois Island, in 35 feet of water are fine beds of *Nitella opaca* and just beyond that Island the bottom drops away to nearly 200 feet. Northward behind Log Bay Island is a sheltered harbor, and on the way into it are some fine mussel beds and then broad shoals densely covered with pipewort.

At the head of Northwest Bay on the west side of the lake, where another mountain stream enters through marshy lowlands there is another rich collecting ground. Here young fishes and minnows are exceedingly abundant, and everything that is needed to fed them is here also. Submerged logs in the stream are plastered over with colonies of the bryozoan, *Pectinatella magnifica*.

At the foot of Black Mountain on the eastern shore is *Chives Rock*, so called from a species of chives (Allium) that is said to grow in the narrow crevices that traverse its broad face. It presents a broad flat surface to the waves and rises directly out of rather deep water. We found this an especially good place to get a line on the lotic insects of

the lake. Many of these, which are not easily collected from the lake, climb up the flat surface of this rock to transform and leave their cast skins clinging there, most of them within reach from a boat anchored at the foot of the vertical cliff. In an hour's collecting I gathered a great number and variety of those belonging to two species of stone-flies, several species of Heptagenine mayflies, three species of dragon-flies, and I noted scores of adult orl-flies, *Sialis infumata*, entangled in the spiders' webs that clung to the face of the rock. The most unexpected find here was four skins of the fine Corduline dragonfly, *Neurocordulia obsoleta*.

Doubtless there are other good collecting grounds on the lake: these are the ones we know about. A single season was not time enough for a very wide acquaintance. The clustered bays about the southeast corner of the lake, where occur the most extensive beds of shore vegetation, and where we did several days' collecting, are doubtless rich fields also.

It seems a bit strange that a body of water that is so easily accessible and that is visited annually by so many thousands of people, should be so little known biologically. Apparently not even a list of its fishes is anywhere available. Yet the islands are mostly state-owned and are offered freely for camping sites. Three passenger boats each way daily with frequent stops make any part of the lake easy of access. A grocer's boat making a circuit of the camps several times a week helps to solve the forage problem; and it would seem that these things should bring more naturalists to this, one of the most beautiful and one of the cleanest of American Lakes.

# THE HISTORY OF SCIENCE AS AN ERROR BREEDER

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WHILE the rôle of the history of science as an error exterminator is much more significant than its rôle as an error breeder, it is necessary to consider it in both of these lights in order to understand its bearings completely. If a comparatively harmless pest destroys pests which are more harmful than itself, it is desirable to consider the injuries which it inflicts as well as the services which it renders. The services that the history of science renders as an error exterminator have received much attention, especially in recent years, but its mischievous rôle as an error breeder seems to have received only little public notice, hence it may be profitable to consider here some striking evidences of this rôle.

In these days of great specialization in science, it is scarcely necessary to emphasize Sir W. Hamilton's dictum "the greater the extension the smaller the intension." All scientists are aware of the fact that it is a very difficult matter to secure a deep and satisfactory intellectual penetration even in a comparatively small domain of science. If one desires to obtain a comprehensive view of the fundamental developments in a larger domain, such as chemistry or mathematics, one has to take much for granted that has been said by others but has not been fully verified by oneself. This method of procedure has to be followed still more by those who strive to secure comprehensive views as regards the fundamental developments in science as a whole.

The highly commendable attitude of mind which seeks to understand the broad lessons taught by science as a whole and to secure a comprehensive view not only of the scientific work now being done in various countries but also of the work done during the preceding ages presents a great contrast when compared with that actuating the extreme specialist when working in a field which he has made his own. It is true that the historian of science is not always actuated by the former attitude of mind. He, too, has his special problems and ponders profoundly over some of the elements in his work. As regards these particular elements he stands on a par with the specialists in other fields.

The historian of science is compelled, however, when he is called upon to treat science as a whole, or even a large domain thereof, to take much for granted which he cannot verify on account of lack of

time. It is here where the breeding of errors makes its appearance. The historian is greatly tempted to state interesting and striking facts. He finds that many such facts have been emphasized by the specialists, but he naturally fails at times in his efforts to interpret the language of the specialists. The next historian who tries to interpret the words of this earlier historian frequently misses the correct interpretaion still more, and hence statements conveying an entirely false notion tend to creep into the general histories of science.

As pure mathematics is the most exact science it may perhaps be assumed that the history of this subject is the most accurate among the histories of the various sciences. At any rate, it seems desirable to illustrate some of the preceding observations by examples of such an elementary type that they can be easily understood by all. The history of mathematics furnishes many such examples since some of its early permanent developments belong to a period when the mathematical specialists dealt with questions which all could easily understand. These specialists had not then raised themselves to great intellectual heights by standing on the shoulders of other specialists, who, in turn, stood on the shoulders of earlier specialists in almost endless succession.

To furnish a striking but somewhat extreme illustration of the fact that the mathematical historian is apt to repeat statements which he does not fully understand, it may be noted here that on page 165 of the third edition of Cantor's well-known *Vorlesungen über Geschichte der Mathematik*, 1907, it is stated that the Greeks used the term *epimorion* to denote the ratio $n/(n+1)$, and that "Archytas had already stated and proved the theorem that if an epimorion, $\alpha/\beta$ is reduced to its lowest terms, which may be called $\mu/\nu$, then $\nu=\mu+1$. It is evident that this remark is practically meaningless, for if $\alpha/\beta$ is an epimorion then it is obviously already in its lowest terms according to the definition of the term epimorion just noted, which seems itself to be incorrect. Notwithstanding this obvious lack of clearness, the statement appears again on page 53 of the second edition of Cajori's *History of Mathematics*, 1919, in spite of the fact that G. Enestrom had in the meantime directed attention to its inaccuracy in volume 8 of the *Bibliotheca Mathematica*, 1917-1918, page 174.

An important feature of the history of science is that many statements made therein are intended to be true only in a general way, while others are supposed to be exact, and the reader has frequently to decide for himself to which of these two classes a particular statement is supposed to belong. For instance, one can usually not determine accurately who was the founder of a large subject since steps towards its development were commonly taken by a number of different men. On the other hand, such statements as "Newton and Leibniz were

the founders of the calculus" have an important historical significance. One might almost say that such incomplete assertions constitute the meat of a general history of science. When it is remembered that they are supposed to represent only first approximations to the truth they can not be regarded in the light of actual error breeders, even if they are apt to have this effect temporarily, especially on beginners.

The significance of many statements in the history of science grows with the growth of the reader's breadth of knowledge relating to the subject involved. Statements which at one period of his scientific development would have appeared satisfactory may not appear so at a later period. It is, of course, impossible for an author of a general history of science to provide for these various stages of development, but there are instances where such authors have failed to provide a satisfactory account for any of their readers irrespective of the stage of their scientific development. While such instances are comparatively rare and do not necessarily impair seriously the works in which they appear they may serve to illustrate the general topic under consideration and are unusually interesting in themselves. Hence we give here one such instance relating to mathematics during medieval times.

On page 314 of Hankel's *Geschichte der Mathematik*, 1874, it is stated that "the first mathematical paper of the Middle Ages which deserves this name is a letter of Gerbert to Adelbold, bishop of Utrecht." In view of the fact that we are now living in an age of numerous scientific papers, this remark by Hankel should be of great interest at the present time. Such interest is reflected in the fact that F. Cajori quotes this remark in the two editions of his *History of Mathematics*, 1894 and 1919, respectively, as well as in the two editions of his *History of Elementary Mathematics*, 1896 and 1917, respectively. Hence the American reader of the history of science is seriously exposed to the danger of assuming that the said remark by Hankel represents a well-established historical fact.

Such an assumption does not imply that the said letter by Gerbert, who died as Pope Sylvester II in 1003, contained any new mathematical results, since there are now expository mathematical papers as well as research papers. It does not imply that the letter in question was long, since there are now many brief mathematical papers as well as long ones. It does, however, imply that this letter was superior to the many other mathematical writings which had appeared during the four or five centuries which had then elapsed since the beginning of the Middle Ages.

It is true that such a superiority even in so long a period of years does not imply very much, since this particular period was unusually barren as regards mathematical developments. Notwithstanding this comparative barrenness, it includes a few noteworthy oases created

especially by the Arabs in algebra and in trigonometry. In particular, the work from which our modern term algebra is derived was composed during this period, and the work of several well-known Hindu writers appeared therein. Hence the reader seems justified for having somewhat high expectations as regards the mathematical importance of Gerbert's letter if it actually deserves being called "the first mathematical paper of the Middle Ages." In fact, the non-mathematical reader might be inclined to fear that the mathematical merits of this letter were too great to lie within the limits of his comprehension.

These expectations and fears are apt to be enhanced by the reading of the accounts of Gerbert's letter in some of our most popular histories of mathematics, including the ones already noted. Not only is it stated here that this letter contained a *correct* explanation for the difference of the results obtained by using two different formulas for the determination of the area of an equilateral triangle, but some of the other statements relating to this letter are sufficiently obscure and misleading to arouse the suspicion that the subject treated therein might possibly be difficult. In various instances the obscurity is increased by the fact that figures of triangles which are not equilateral are given, while the text relates to an equilateral triangle. This is done, for instance, on page 249 of Günther's, *Geschichte der Mathematik*, 1908, as well as in the three editions of volume I of Cantor's well known *Vorlesungen über Geschichte der Mathematik*, pages 744, 815 and 866, respectively.

From the preceding remarks the reader will naturally conclude that the present writer does not believe that the letter in question merits to be called "the first mathematical paper of the Middle Ages which deserves this name," notwithstanding the fact that this epithet has been applied to it by eminent authoritities. In fact, the present writer believes not only that the letter does not merit this epithet but that it is of so little mathematical importance as to make it appear ridiculous to make such a claim for it. Moreover, he believes that other statements made about this letter in well-known mathematical histories are strikingly inaccurate. In order to establish the correctness of this point of view, it is necessary to state just what is found in the part of this letter which has been preserved, upon which our view of its merits must be based.

In view of the great claims made for this letter, the reader will naturally be surprised to find that it deals with the very elementary question of finding the area of an equilateral triangle, a question which had been completely solved many centuries before. Gerbert gives here the rule that the altitude of such a triangle can be found by subtracting one-seventh from its side, which is a sufficiently close approximation for many purposes, since the altitude is $^{a}/_{2}\sqrt{3}$, where $a$ is the

side. He then states, in substances, that the area of an equilateral triangle is one-half of the product of the base and the altitude thus obtained, and he calls this the "geometric rule" for finding such an area.

Thus far there is nothing surprising in this letter, and no one seems to have claimed much credit for this part, but Gerbert then makes some very inaccurate and foolish remarks about finding the area of such a triangle by another rule, called the "arithmetic rule," and it is just upon these remarks which exhibit a great lack of geometric insight that the high claims of this letter have been based. The inaccuracy of these remarks had been noted by M. Chasles in his well and favorably known *Aperçu Historique*, 1875, page 506, but notwithstanding this fact various later mathematical authors, including all those noted above, have called them correct in their general histories.

In order to appreciate the crudeness of this "arithmetic rule," which is equivalent to the formula $\frac{1}{2}a(a+1)$, it may be noted that in the work of the Egyptian Ahmes, written about 1700 B. C., the area of an isosceles triangle seems to have been found by multiplying one-half the base by a side instead of by the altitude. This method has been regarded as remarkable on account of its crudity, but when we are told that more than two thousand years later the Roman surveyors were taught to find the area of such a triangle by finding the product of one-half of the numerical measure of the base and a number which is even larger than the numerical measure of another side, there seems to be sufficient ground for surprise even in a scientific matter.

It must be admitted that the instances cited above are insufficient to establish the fact that a general history of science is an unusually favorable ground for the breeding and the propagation of scientific errors. In fact, all that has been attempted here is to advance a few reasons why one might suspect danger here, and to support these reasons by illustrations which were assumed to be also of interest to the reader on account of their unusual intrinsic features. Perhaps a more conclusive argument in support of the thesis in question is furnished by the fact that G. Eneström noted more than two thousand desirable changes relating to the general history of mathematics by M. Cantor, to which reference was made.

# THE BIOLOGY OF DEATH. III—THE CHANCES OF DEATH[1]

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## 1. The Life Table

UP to this point in our discussion of death and longevity we have, for the most part, dealt with general and qualitative matters, and have not made any particular examination as to the quantitative aspects of the problem of longevity. To this phase attention may now be directed. For one organism, and one organism only, do we know much about the quantitative aspects of longevity. I refer, of course, to man, and the abundant records which exist as to the duration of his life under various conditions and circumstances. In 1532 there began in London the first definitely known compilation of weekly "Bills of Mortality." Seven years later the official registration of baptisms, marriages and deaths was begun in France, and shortly after the opening of the seventeenth century similar registration was begun in Sweden. In 1662 was published the first edition of a remarkable book, a book which marks the beginning of the subject which we now know as "vital statistics." I refer to "Natural and Political Observations Mentioned in the Following Index, and made upon the Bills of Mortality" by Captain John Graunt, Citizen of London. From that day to this, in an ever widening portion of the inhabited globe we have had more or less continuous published records about the duration of life in man. The amount of such material which has accumulated is enormous. We are only at the beginning, however, of its proper mathematical and biological analysis. If biologists had been furnished with data of anything like the same quantity and quality for any other organism than man one feels sure that a vastly greater amount of attention would have been devoted to it than ever has been given to vital statistics, so-called, and there would have been as a result many fundamental advances in biological knowledge now lacking, because material of this sort so generally seems to the professional biologist to be something about which he is in no way concerned.

Let us examine some of the general facts about the normal duration of life in man. We may put the matter in this way: Suppose we started out at a given instant of time with a hundred thousand infants.

[1]Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University, No. 30.

equally distributed as to sex, and all born at the same instant of time. How many of these individuals would die in each succeeding year, and what would be the general picture of the changes in this cohort with the passage of time? The facts on this point for the Registration Area of the United States in 1910 are exhibited in Figure 1, which is based on Glover's United States Life Tables.

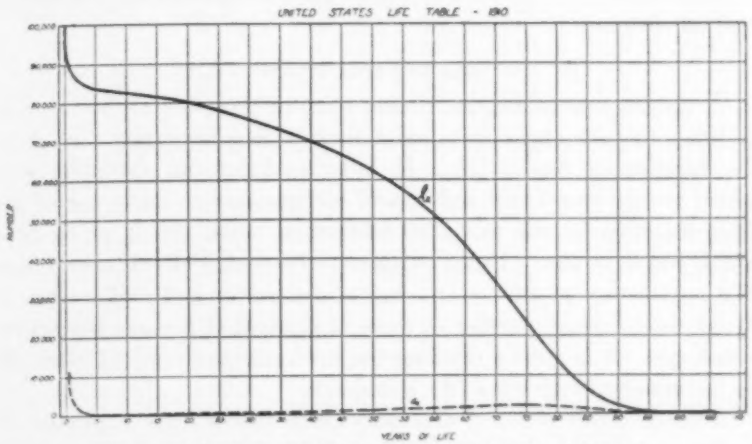


FIG. 1. LIFE TABLE DIAGRAM. FOR EXPLANATION SEE TEXT

In this table are seen two curved lines, one marked $l_x$ and the other $d_x$. The $l_x$ line indicates the number of individuals, out of the original 100,000 starting together at birth, who survived at the beginning of each year of the life span, indicated along the bottom of the diagram. The $d_x$ line shows the number dying within each year of the life span. In other words, if we subtract the number dying within each year from the number surviving at the beginning of that year we shall get the series of figures plotted as the $l_x$ line. We note that in the very first year of life the original hundred thousand lose over one-tenth of their number, there being only 88,538 surviving at the beginning of the second year of life. In the next year 2,446 drop out, and in the year following that 1,062. Then the line of survivors drops off more slowly between the period of youth and early adult life. At 40 years of age, almost exactly 30,000 of the original 100,000 have passed away, and from that point on the $l_x$ line descends with ever increasing rapidity, until about age 80, when it once more begins to drop more slowly, and the last few survivors pass out gradually, a few each year until something over the century mark is reached, when the last of the 100,000 who started so blithely across the bridge of life together will have ended his journey.

This diagram is a graphic representation of that important type of document known as a life or mortality table. It puts the facts of mortality and longevity in their best form for comparative purposes. The

first such table actually to be computed in anything like the modern fashion was made by the astronomer, Dr. E. Halley, and was published in 1693. Since that time a great number of such tables have been calculated. Dawson fills a stout octavo volume with a collection of the more important of such tables computed for different countries and different groups of the population. Now they have become such a commonplace that elementary classes in vital statistics are required to compute them.

## 2. Changes in Expectation in Life

I wish to pass in graphic review some of these life tables in order to bring to your attention in vivid form a very important fact about the duration of human life. In order to bring out the point with which we are here concerned it will be necessary to make use of another function of the mortality table than either the $l_x$ or $d_x$ lines which you have seen. I wish to discuss expectation of life at each age. The expectation of life at any age is defined in actuarial science as the mean or average number of years of survival of persons alive at the stated age. It is got by dividing the total survivor-years of after life by the number surviving at the stated age.

In each of the series of diagrams which follow there is plotted the approximate value of the expectation of life for some group of people at some period in the more or less remote past, and for comparison the expectation of life either from Glover's table, for the population of the United States Registration Area in 1910—the expectation of life of our people now, in short—or equivalent figures for a modern English population.

Because of the considerable interest of the matter, and the fact that the data are not easily available to biologists, Table 1 is inserted giving the expectations of life from which the diagrams have been plotted.

TABLE I.

*Changes in expectation of life from the seventeenth century to the present time.*

| Age | Average length of life remaining to each one alive at beginning of age interval. | | | Age | Average length of life remaining to each one alive at beginning of age interval. | | |
|---|---|---|---|---|---|---|---|
| | Breslau, 17th century. | Carlisle, 18th century. | U. S. 1910 | | Breslau, 17th century. | Carlisle, 18th century. | U. S. 1910 |
| 0 - 1 | 33.50 | 38.72 | 51.49 | 50-51 | 16.81 | 21.11 | 20.98 |
| 1 - 2 | 38.10 | 44.67 | 57.11 | 51-52 | 16.36 | 20.39 | 20.28 |
| 2 - 3 | 39.78 | 47.55 | 57.72 | 52-53 | 15.92 | 19.68 | 19.58 |
| 3 - 4 | 40.75 | 49.81 | 57.44 | 53-54 | 15.48 | 18.97 | 18.89 |
| 4 - 5 | 41.25 | 50.76 | 56.89 | 54-55 | 14.99 | 18.27 | 18.21 |
| 5 - 6 | 41.55 | 51.24 | 56.21 | 55-56 | 14.51 | 17.58 | 17.55 |
| 6 - 7 | 41.62 | 51.16 | 55.47 | 56-57 | 14.02 | 16.89 | 16.90 |
| 7 - 8 | 41.16 | 50.79 | 54.69 | 57-58 | 13.54 | 16.21 | 16.26 |
| 8 - 9 | 40.95 | 50.24 | 53.87 | 58-59 | 13.06 | 15.55 | 15.64 |
| 9 -10 | 40.50 | 49.57 | 53.02 | 59-60 | 12.57 | 14.92 | 15.03 |
| 10-11 | 39.99 | 48.82 | 52.15 | 60-61 | 12.09 | 14.34 | 14.42 |
| 11-12 | 39.43 | 48.04 | 51.26 | 61-62 | 11.62 | 13.82 | 13.83 |
| 12-13 | 38.79 | 47.27 | 50.37 | 62-63 | 11.14 | 13.31 | 13.26 |
| 13-14 | 38.16 | 46.50 | 49.49 | 63-64 | 10.67 | 12.81 | 12.69 |
| 14-15 | 37.51 | 45.74 | 48.60 | 64-65 | 10.20 | 12.30 | 12.14 |
| 15-16 | 36.86 | 44.99 | 47.73 | 65-66 | 9.73 | 11.79 | 11.60 |
| 16-17 | 36.22 | 44.27 | 46.86 | 66-67 | 9.27 | 11.27 | 11.08 |
| 17-18 | 35.57 | 43.57 | 46.01 | 67-68 | 8.81 | 10.75 | 10.57 |
| 18-19 | 34.92 | 42.87 | 45.17 | 68-69 | 8.36 | 10.23 | 10.07 |
| 19-20 | 34.26 | 42.16 | 44.34 | 69-70 | 7.91 | 9.70 | 9.58 |
| 20-21 | 33.61 | 41.46 | 43.53 | 70-71 | 7.53 | 9.17 | 9.11 |
| 21-22 | 32.95 | 40.75 | 42.73 | 71-72 | 7.17 | 8.65 | 8.66 |
| 22-23 | 32.34 | 40.03 | 41.94 | 72-73 | 6.85 | 8.16 | 8.22 |
| 23-24 | 31.67 | 39.31 | 41.16 | 73-74 | 6.56 | 7.72 | 7.79 |
| 24-25 | 31.00 | 38.58 | 40.38 | 74-75 | 6.25 | 7.33 | 7.38 |
| 25-26 | 30.38 | 37.86 | 39.60 | 75-76 | 5.99 | 7.00 | 6.99 |
| 26-27 | 29.76 | 37.13 | 38.81 | 76-77 | 5.79 | 6.69 | 6.61 |
| 27-28 | 29.14 | 36.40 | 38.03 | 77-78 | 5.71 | 6.40 | 6.25 |
| 28-29 | 28.51 | 35.68 | 37.25 | 78-79 | 5.66 | 6.11 | 5.90 |
| 29-30 | 27.93 | 34.99 | 36.48 | 79-80 | 5.67 | 5.80 | 5.56 |
| 30-31 | 27.35 | 34.34 | 35.70 | 80-81 | 5.74 | 5.51 | 5.25 |
| 31-32 | 26.76 | 33.68 | 34.93 | 81-82 | 5.86 | 5.20 | 4.96 |
| 32-33 | 26.18 | 33.02 | 34.17 | 82-83 | 6.02 | 4.93 | 4.70 |
| 33-34 | 25.59 | 32.36 | 33.41 | 83-84 | 5.85 | 4.65 | 4.45 |
| 34-35 | 25.05 | 31.68 | 32.66 | 84-85 | | 4.39 | 4.22 |
| 35-36 | 24.51 | 31.00 | 31.90 | 85-86 | | 4.12 | 4.00 |
| 36-37 | 23.97 | 30.32 | 31.16 | 86-87 | | 3.90 | 3.79 |
| 37-38 | 23.43 | 29.63 | 30.42 | 87-88 | | 3.71 | 3.58 |
| 38-39 | 22.88 | 28.95 | 29.68 | 88-89 | | 3.59 | 3.39 |
| 39-40 | 22.33 | 28.27 | 28.94 | 89-90 | | 3.47 | 3.20 |
| 40-41 | 21.78 | 27.61 | 28.20 | 90-91 | | 3.28 | 3.03 |
| 41-42 | 21.23 | 26.97 | 27.46 | 91-92 | | 3.26 | 2.87 |
| 42-43 | 20.73 | 26.33 | 26.73 | 92-93 | | 3.37 | 2.73 |
| 43-44 | 20.23 | 25.71 | 25.99 | 93-94 | | 3.48 | 2.59 |
| 44-45 | 19.72 | 25.08 | 25.26 | 94-95 | | 3.53 | 2.47 |
| 45-46 | 19.22 | 24.45 | 24.54 | 95-96 | | 3.53 | 2.35 |
| 46-47 | 18.72 | 23.81 | 23.82 | 96-97 | | 3.46 | 2.24 |
| 47-48 | 18.21 | 23.16 | 23.10 | 97-98 | | 3.28 | 2.14 |
| 48-49 | 17.71 | 22.50 | 22.39 | 98-99 | | 3.07 | 2.04 |
| 49-50 | 17.25 | 21.81 | 21.69 | 99-100 | | 2.77 | 1.95 |

Figure 2 gives the results from Halley's table, based upon the mortality experience in the City of Breslau, in Silesia, during the years 1687 to 1691. This gives us a picture of the forces of mortality towards

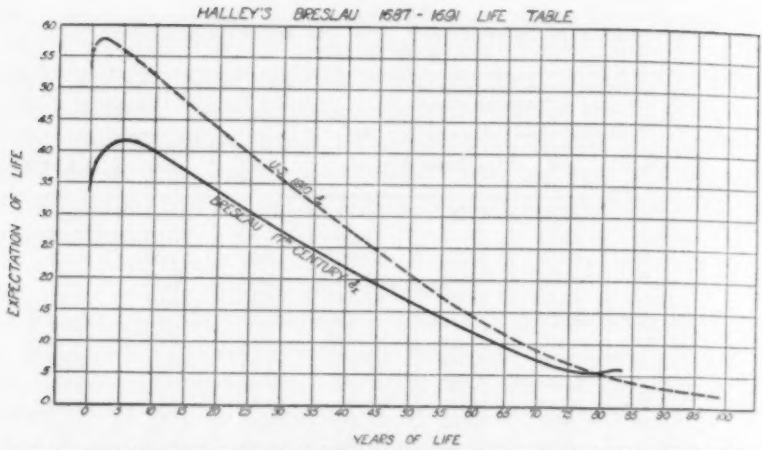


FIG. 2. COMPARING THE EXPECTATION OF LIFE IN THE 17TH CENTURY WITH THAT OF THE PRESENT TIME

the end of the seventeenth century. From this diagram it appears that at birth the expectation of life of an individual born in Breslau in the seventeenth century was very much lower than that of an individual born in the United States in 1910. The difference amounts to approximately 18 years! At 10 years of age, however, this difference in expectation of life had been reduced to just over 12 years; at age 20, to a little less than 10 years; at age 30 to 7-1/3 years; at age 50 to just over 4 years; at age 70 to 1-1/2 years. At age 80 the lines have crossed. The individual 80 years old in Breslau could expect to live on the average a half year *longer* than the individual of the same age in the United States in 1910. At age 83, the last year covered by Halley's table, the 17th century individual could expect on the average to live approximately a year and a half longer than his twentieth century brother. So then what the diagram shows is that the expectation of life at early ages was vastly inferior in the seventeenth century to what it is now, while at advanced ages the chances of living were distinctly better—*relatively* enormously better—then than they are now. Let us defer the further discussion of the meaning and explanation of this curious fact until we have examined some further data.

Figure 3 compares the expectation of life in England at the middle of the eighteenth century, or about a century later than the last, with present conditions in the United States. Again we see that the expectation at birth was greatly inferior then to what it is now, but the difference is not so great as it was a century earlier, amounting to but 12-3/4 years instead of the 18 we found before. Further it is seen that, just as before, the expectations come closer together with advancing age. By the time age 45—middle life—is reached the expectation of life was substantially the same in the eighteenth century as it is now. At age 47 the eighteenth century line crosses that for the twentieth century,

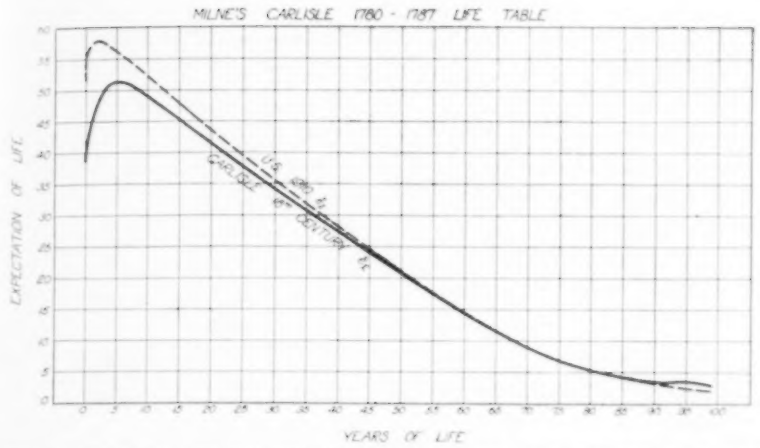


FIG. 3. COMPARING THE EXPECTATION OF LIFE IN THE 18TH CENTURY WITH THAT OF THE PRESENT TIME

and with a few trifling exceptions, notably in the years from 56 to 62, the expectation of life for all higher ages was greater then than it is now. Or we see in the eighteenth century the same *kind* of result as in the seventeenth, only differing in degree.

The changes in expectation of life from the middle of the seventeenth century to the present time furnish a record of a real evolutionary progression. In this respect at least man has definitely and distinctively changed, as a race, in a period of three and a half centuries. This is, of course, a matter of extraordinary interest, and at once stimulates the desire to go still farther back in history and see what the expectation of life then was. Fortunately, through the labors of Karl Pearson, and his associate, W. R. Macdonell, it is possible to do this, to at least a first approximation. Pearson has analyzed the records as to age at death which were found upon mummy cases studied by Professor W. Spiegelberg. These mummies belonged to a period between 1900 and 2000 years ago, when Egypt was under Roman dominion. The data were extremely meager, but from Pearson's analysis of them it has been possible to construct the diagram which is shown in Figure 4. Each circle marks a point where it was possible definitely to calculate an expectation of life. The curve running through the circles is a rough graphic smoothing of the scattered observed data. Unfortunately, there were no records of deaths in early infancy. Either there were no baby mummies, or if there were they have disappeared. For comparison, the expectation of life from Glover's 1910 United States life table is inserted.

It will be seen at once that the general sweep of the line is of the same sort that we have already observed in the case of the seventeenth century table. In the early years of life the expectation was far below that of the present time, but somewhere between ages 65 and 70 the

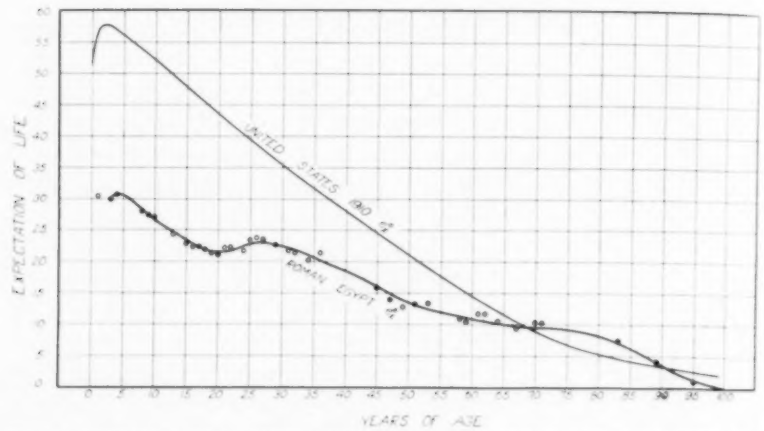


FIG. 4. COMPARING THE EXPECTATION OF LIFE OF ANCIENT EGYPTIANS WITH THAT OF PRESENT DAY AMERICANS. Plotted from Pearson's and Glover's data

Egyptian line crosses the modern American line, and from that period on the individuals living in Egypt at about the time of the birth of Christ could look forward to a longer remaining duration of life, on the average, than can the American of the present day. Pearson's comment on this fact is worth quoting. He says: "In the course of those centuries man must have grown remarkably fitter to his environment, or else he must have fitted his environment immeasurably better to himself. No civilized community of to-day could show such a curve as the civilized Romano-Egyptians of 2,000 years ago exhibit. We have here either a strong argument for the survival of the physically fitter man or for the survival of the civilly fitter society. Either man is constitutionally fitter to survive to-day, or he is mentally fitter, i. e., better able to organize his civic surroundings. Both conclusions point perfectly definitely to an evolutionary progress. . . . That the expectation of life for a Romano-Egyptian over 68 was greater than for a modern English man or woman is what we might expect, for with the mortality of youth and of middle age enormously emphasized only the very strongest would survive to this age. Out of 100 English alive at 10 years of age 39 survive to be 68; out of 100 Romano-Egyptians not 9 survived. Looking at these two curves we realize at a glance either the great physical progress of man, which enables him far more effectually to withstand a hostile environment, or the great social and sanitary progress he has made which enables him to modify the environment. In either case we can definitely assert that 2,000 years has made him a much 'fitter' being. In this comparison it must be remembered that we are not placing a civilized race against a barbaric tribe, but comparing a modern civilization with one of the highest types of ancient civilization."

Macdonell was able to continue this investigation, on much more

extensive material extracted from the *Corpus Inscriptionum Latinarum* of the Berlin Academy, which gives records as to age of death for many thousand Roman citizens dying, for the most part, within the first three or four centuries of the Christian era. His material may, therefore, be taken to represent the conditions a few centuries later than those of Pearson's Romano-Egyptian population. Macdonell was able to calculate three tables of expectation of life—the first for Roman citizens living in the city of Rome itself; second, for those living in the provinces of Hispania and Lusitania; and third, for those living in Africa. The results are plotted against the United States 1910 data, as before, in Figures 5, 6 and 7.

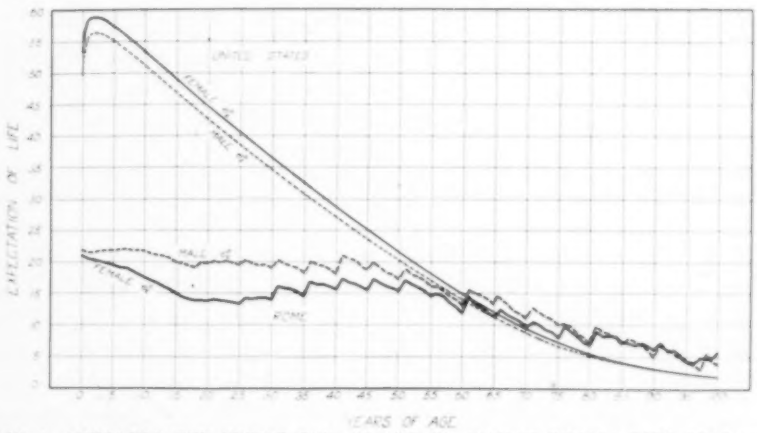


FIG. 5. COMPARING THE EXPECTATION OF LIFE OF ANCIENT ROMANS WITH THAT OF PRESENT DAY AMERICANS. Plotted from Macdonell's and Glover's data

Figure 5 relates to inhabitants of the city of Rome itself. The populations from which the expectations are calculated run into the thousands, and fortunately one is able to separate males and females. As in Pearson's case, which we have just examined, modern American data are entered for comparison. It will be noted at once that just as in the Romano-Egyptian population the expectation of life of inhabitants of ancient Rome was, in the early years of life, immensely inferior to that of the modern population. From about age 60 on, however, the expectation of life was better then than now. Curiously enough, the expectation of life of females was poorer at practically all ages of life than that of the males, which exactly reverses the modern state of affairs. Macdonell believes this difference to be real, and to indicate that there were special influences adversely affecting the health of females in the Roman Empire, which no longer operate in the modern world. Up to something like age 25 the expectation of life of dwellers in the city of Rome was extremely bad, worse than in the Romano-Egyptian population which Pearson studied, or in the popu-

lations of other parts of the Roman Empire as we shall see in the following diagram. Macdonell thinks that this difference is real and due to circumstances peculiar to Rome.

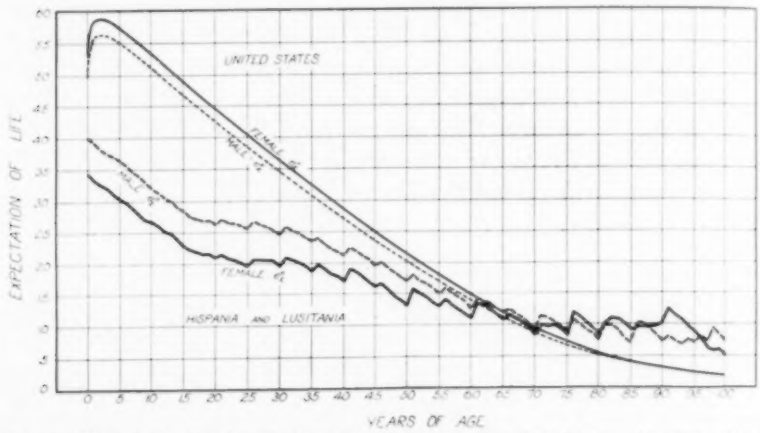


FIG. 6. COMPARING THE EXPECTATION OF LIFE OF THE POPULATION OF THE ROMAN PROVINCES HISPANIA AND LUSITANIA WITH THAT OF PRESENT DAY AMERICANS. Plotted from Macdonell's and Glover's data

The general features of the diagram for the population of Hispania and Lusitania (Figure 6) are similar to those that we have seen, with the difference that the expectation of life up to age 20 or 25 is not as bad as in the city of Rome itself. Again the females show a lower expectation practically throughout life than do the males. The lines cross the modern American lines at about age 60 and from that point on these colonial Romans had a better expectation of life than the modern American has.

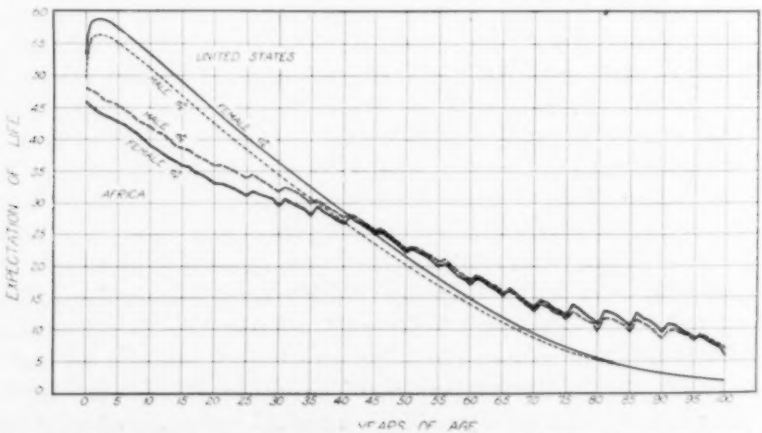


FIG. 7. COMPARING THE EXPECTATION OF LIFE OF THE POPULATION OF THE ROMAN PROVINCES IN AFRICA WITH THAT OF PRESENT DAY AMERICANS. Plotted from Macdonell's and Glover's data

The Romano-African population diagram appears to start at nearly the same point at birth as does the modern American and in general the differences up to age 35 are not substantially more marked from modern conditions than they are in the seventeenth century Breslau table. The striking thing, however, is that at about age 40 the lines cross, and from then on the expectation of life was definitely superior in the early years of the Christian era to what it is now.

It should be said that the curious zigzagging of the lines in all of these Roman tables of Macdonell is due to the tendency, which ancient Romans apparently had in common with present day American negroes, towards heavy grouping on the even multiples of 5 in the statement of their ages.

Summarizing the whole matter we see that during a period of approximately 2,000 years man's expectation of life at birth and subsequent early ages has been steadily improving, while at the same time his expectation of life at advanced ages has been steadily worsening. The former phenomenon may be attributed essentially to ever increasing knowledge of how best to cope with the lethal forces of nature. Progressively better sanitation, in the broadest sense, down through the centuries has saved for a time the lives of ever more and more babies and young people who formerly could not withstand the unfavorable conditions they met, and died in consequence rather promptly. But just because this process tends to preserve the weaklings, who were speedily eliminated under the rigorous action of unmitigated natural selection, there appear now in the higher age groups of the population many weaker individuals than formerly ever got there. Consequently the average expectation of life at ages beyond say 60 to 70 is not nearly so good now as it was under the more rigorous régime of ancient Rome. Then any individual who attained age 70 was the surviving resultant of a bitterly destructive process of selection. To run successfully the gauntlet of early and middle life he necessarily had to have an extraordinarily vigorous and resistant constitution. Having come through successfully to 70 years of age it is no matter of wonder that his prospects were for a longer old age than his descendants of the same age to-day can look forward to. Biologically these expectation of life curves give us the first introduction to a principle which we shall find as we go on to be of the very foremost importance in fixing the span of human longevity, namely that *inherited constitution fundamentally and primarily determines how long an individual will live.*

### 3. Analysis of the Life Table

I shall not develop this point further now, but instead will turn back to consider briefly certain features of the $d_x$ line of a life table. Figure 1 shows that this line, which gives the number of deaths occur-

ring at each age, has the form of a very much stretched letter S resting on its back. Some years ago Pearson undertook the analysis of this complex curve, and drew certain interesting conclusions as to the fundamental biological causes lying behind its curious sinuosity. His results are shown in Figure 8.

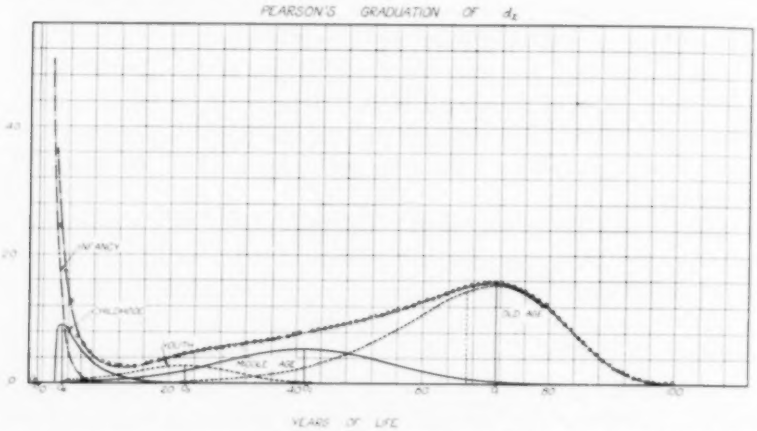


FIG. 8. SHOWING PEARSON'S RESULTS IN FITTING THE $Dx$ LINE OF THE LIFE TABLE WITH 5 SKEW FREQUENCY CURVES. Plotted from the data of Pearson's original memoir on "Skew Variation" in the Phil. Trans. Roy. Soc.

He regarded the $d_x$ line of the life table as a compound curve, and by suitable mathematical analysis broke it up into five component frequency curves. The data which he used were furnished by the $d_x$ line of Ogle's life table, based on the experience of 1871 to 1880 in England. This line gives the deaths per annum of one thousand persons born in the same year. The first component which he separated was the old age mortality. This is shown by the dotted curve having its modal point between 70 and 75 years, at the point lettered $O_1$ on the base of the diagram. This component, according to Pearson's graduation, accounted for 484.1 deaths out of the total of 1,000, or nearly one-half of the whole. Its range extends from under 20 years of age to the upper limit of life, at approximately 106 years. The second component includes the deaths of middle life. This is the smooth curve having its modal point between 40 and 45 years at the point on the base marked $O_2$. Its range extends from about 5 years of age to about 65. It accounts for 175.2 deaths out of the total of 1,000. It is a long, much spread out curve, exhibiting great variability. The third component is made up by the deaths of youth. This accounts for 50.8 deaths out of the total of a thousand, and its range extends from about the time of birth to nearly 45 years. Its mid-point is between 20 and 25 years, and it exhibits less variability than either the middle life or the old age curves. The fourth component, the modal point of which is at the point on the base of the diagram marked $O_4$ covers the childhood

mortality. It accounts for 46.4 deaths out of the total of 1,000. Its range and variability are obviously less than those of any of the other three components so far considered. The last, excessively skew component, is that which describes the mortality of infancy. It is given by a J shaped curve accounting for 245.7 deaths after birth, and an antenatal mortaliy of 605. In order to get any fit at all for this portion of the mortality curve it is necessary to assume that the deaths *in utero* and those of the first months after birth are a homogeneous connected group.

Summing all these components together it is seen that the resulting smooth curve very closely fits the series of small circles which are the original observations. From the standpoint merely of curve fitting no better result than this could be hoped for. But about its biological significance the case is not quite so clear, as we shall presently see.

Pearson himself thinks of these five components of the mortality curve as typifying five Deaths, shooting with different weapons, at different speeds and with differing precision at the procession of human beings crossing the Bridge of Life. The first Death is, according to Pearson, a marksman of deadly aim, concentrated fire, and unremitting destructiveness. He kills before birth as well as after and may be conceived as beating down young lives with the bones of their ancestors. The second marksman who aims at childhood has an extremely concentrated fire, which may be typified by the machine gun. Only because of the concentration of this fire are we able to pass through it without appalling loss. The third marksman Death, who shoots at youth has not a very deadly or accurate weapon, perhaps a bow and arrow. The fire of the fourth marksman is slow, scattered and not very destructive, such as might result from an old fashioned blunderbus. The last Death plies a rifle. None escapes his shots. He aims at old age but sometimes hits youth. His unremitting activity makes his toll large.

We may let Pearson sum the whole matter up in his own words: "Our investigations on the mortality statistics have thus led us to some very definite conclusions with regard to the chances of death. Instead of seven we have five ages of man, corresponding to the periods of infancy, of childhood, of youth, of maturity or middle age, and of senility or old age. In the case of each of these periods we see a perfectly regular chance distribution, centering at a given age, and tailing off on either side according to a perfectly clear mathematical law. . .

"Artistically, we no longer think of Death as striking chaotically; we regard his aim as perfectly regular in the mass, if unpredictable in the individual instance. It is no longer the Dance of Death which pictures for us Death carrying off indiscriminately the old and young, the rich and the poor, the toiler and the idler, the babe and its grandsire. We see something quite different, the cohort of a thousand tiny mites

starting across the Bridge of Life, and growing in stature as they advance, till at the far end of the bridge we see only the gray-beard, and the 'lean and slippered pantaloon.' As they pass along the causeway the throng is more and more thinned; five Deaths are posted at different stages of the route longside the bridge, and with different skewness of aim and different weapons of precision they fire at the human target, till none remains to reach the end of the causeway—the limit of life."

This whole, somewhat fanciful, conception of Pearson's needs a little critical examination. What actually he has done is to get a good empirical fit of the $d_x$ line by the use of equations involving all told some 17 constants. Because the combined curve fits well, *and fundamentally for no other reason*, he implicitly concludes that the fact that the fit is got by the use of five components means biologically that the $d_x$ line is a compound curve, and indicates a five-fold biological heterogeneity in the material. But it is a very hazardous proceeding to draw biological conclusions of this type from the mere fact that a theoretical mathematical function or functions fits well a series of observational data. I have fully discussed this point several years ago (Pearl: Amer. Nat. Vol. XLIII) where I pointed out:

"The kind of evidence under discussion can at best have but inferential significance; it can never be of demonstrative worth. It is based on a process of reasoning which assumes a fundamental or necessary relationship to exist between two sets of phenomena because the same curve describes the quantitative relations of both sets. A little consideration indicates that this method of reasoning certainly can not be of general application, even though we assume it to be correct in particular cases. The difficulty arises from the fact that the mathematical functions commonly used with adequate results in physical, chemical, biological, and mathematical investigations are comparatively few in number. The literature of science shows nothing clearer than that the same type of curve frequently serves to describe with complete accuracy the quantitative relations of widely different natural phenomena. As a consequence any proposition to include that two sets of phenomena are casually or in any other way fundamentally related solely because they are described by the same type of curve is of a very doubtful validity."

Henderson has put Pearson's five components together in a single equation, and says regarding this method of analyzing the life tables: " . . . it is difficult to lay a firm foundation for it, because *no analysis of the deaths into natural divisions by causes or otherwise has yet been made* such that the totals in the various groups would conform to those frequency curves." The italics in this quotation are the present writer's for the purpose of emphasizing crucial points of the whole matter, which we shall immediately discuss in more detail.

Now it is altogether probable that one could get just as good a fit to the observed $d_x$ line as is obtained by Pearson's five components by using a 17 constant equation of the type

$$y=a+bx+cx^2+dx^3+ex^4+fx^5+gx^6+\cdots\cdots\cdots+nx^{16}$$

and in that event one would be quite as fully justified (or really unjustified) in concluding that the $d_x$ line was a homogeneous curve as Pearson is in concluding from his five-component fit that it is compound.

Indeed Wittstein's formula involving but four constants gives substantially good fit over the whole range of life.

But in neither case is the curve-fitting evidence, by and of itself, in any sense a demonstration of the biological homogeneity or heterogeneity of the material. Of far greater importance, and indeed conclusive significance, is the fact, to be brought out in a later paper in this series, that in material *experimentally known to be biologically homogeneous*, a population made up of full brothers and sisters out of a brother x sister mating and kept throughout life in a uniform environment identical for all individuals, *one gets a $d_x$ line in all its essential features*, save for the absence of excessive infant mortality arising from perfectly clear biological causes, *identical with the human $d_x$ line.* It has long been apparent to the thoughtful biologist that there was not the slightest biological reason to suppose that the peculiar sinuosity of the human $d_x$ line owed its origin to any fundamental heterogeneity in the material, or differentiation in respect of the forces of mortality. Now we have experimental proof, to be discussed in a later paper in this series, that with complete homogeneity of the material, both genetic and environmental, one gets just the same kind of $d_x$ line as in normal human material. We must then, I think, come to the conclusion that brilliant and picturesque as is Pearson's conception of the five Deaths, actually there is no slightest reason to suppose that it represents any *biological* reality, save in the one respect that his curve fitting demonstrates, as any other equally successful would, that deaths do not occur chaotically in respect of age, but instead in a regular manner capable of representation by a mathematical function of age.

# PUBLISHED FIGURES AND PLATES OF THE EXTINCT PASSENGER PIGEON[1]

By Dr. R. W. SHUFELDT
WASHINGTON, D. C.

(Photographs by the Author)

WITH the view of portraying its natural appearance in life, few birds, either living or extinct, have exceeded the Passenger Pigeon (*Ectopistes migratorius*) as a subject for artists and engravers. For nearly two centuries, representations of this now extinct species have been published in all sorts of avenues, ranging all the way from the cuts found in dictionaries and school-books to reproductions of life-size colored figures illustrating the most sumptuous of the world's great works devoted to ornithology. It would seem to be quite a safe statement to make that upwards of five hundred figures or more of this bird, published in many quarters of the world, have appeared, illustrating the great variety of accounts, both popular and scientific, that avian biographers have given us upon its natural history.

No species of bird known to man, in all time, can in any way rival the extraordinary series of chapters that go to make up the history of the life-span of this now totally extinct pigeon. As a story filled with romance, prodigality, cruelty and short-sightedness, it outranks the most unbelievable fables of the ancients. For one among many who witnessed the marvelous flight of these birds in the early seventies, I never for a moment thought how soon the species would be in the same category with those other birds, of which the world shall never again see living specimens. We can now only regretfully look back on the picture and systematize the data at hand with respect to the literary part of this, and not a little has been accomplished by those competent to undertake it. But with all this we have nothing to do here, as it is a subject quite apart from a consideration of what we have by way of portraits of a form that man shall never see again in life.

As just stated, there is a very extensive array of these portraits in the many biographies that have appeared of the bird, and they represent a great variety of grades of excellence, of caricature, of faithfulness, and of grotesqueness. Many of these will here be ignored, as they contribute nothing of any value in aiding one to correctly visualize our subject; indeed, in most instances, such cuts convey a decidedly

[1]Read at the Thirty-eighth Stated Meeting of the American Ornithologists' Union, Washington, D. C., November, 1920.

FIG. 1. AN OLD WOOD ENGRAVING FROM THE "ILLUSTRATED NATURAL HISTORY OF THE ANIMAL KINGDOM" BY L. G. GOODRICH

FIG. 2. AN IMMATURE WILD PIGEON. FROM A PHOTOGRAPH BY THE LATE DR. C. O. WHITMAN

erroneous idea as to how this bird appeared in life. This statement may be relied upon coming from me, as not only have I shot a number of specimens of them and handled them afterwards, but I have seen them, close to, in all their natural attitudes in the forest; so that, with such opportunities, added to what faculties I may possess for memorizing the normal postures of birds in life, following upon a study of any particular species of them for that purpose, I may be more or less competent to judge of the faithfulness of portrayal in any picture of the wild pigeon which, up to the present time, has been published.

Turning first, then, to a few of the minor cuts that have appeared of this bird, it is to be noted that they are of a great variety, and based on all sorts of data. Some are reduced woodcuts, or electros, or halftones, made from the large plates in the standard works of the world's recognized ornithologists. Some are fanciful pictures reproduced from drawings made by those who knew nothing of the wild pigeon, or who had examined the figures or plates of others possessing more or less reliable data upon which to base such productions. Not a few are represented by excellent examples of pictorial piracy, with widely varying success as to correctness of copy; in the case of still others, attempts have been made to conceal the piracy, and the value of the result rests upon the skill of the artist to succeed in such a trick. In a few instances, the pirated picture appears to be truer to nature than the one from which it was copied. And, again, such copies are duly acknowledged, either under the cut or in the text which accompanies it.

L. G. Goodrich published his "Illustrated Natural History of the Animal Kingdom" in 1861; it carried 1,500 engravings in the two volumes, and came off the presses of Derby and Jackson, of New York City. On page 231 of Volume II. there is an attractive woodcut of the wild pigeon engraved by Lossing and Barritt. (Fig. 1.) In the foreground a single adult bird faces to the right, standing on the trunk of a fallen tree; in the middle distance there are three more of these birds in a tree to the left, while in the background we have a man, partly concealed in a "blind," netting pigeons. Numerous birds are on the ground; others are in a near-by tree, while still others are coming down to the lure, and a few others are, apparently, for the moment passing in the form of an acute angle, with one bird directly behind another in the two lines forming it.

Whether this is the place where this picture was first published, I am unable to state; but I am inclined to believe that it is not, for the reason that we find, in Thomas Nuttall's "A Manual of the Ornithology of the United States," a picture of a wild pigeon which is evidently the counterpart of the one in Goodrich. Here it is larger, however, and the bird is turned to the left; the surroundings are changed some-

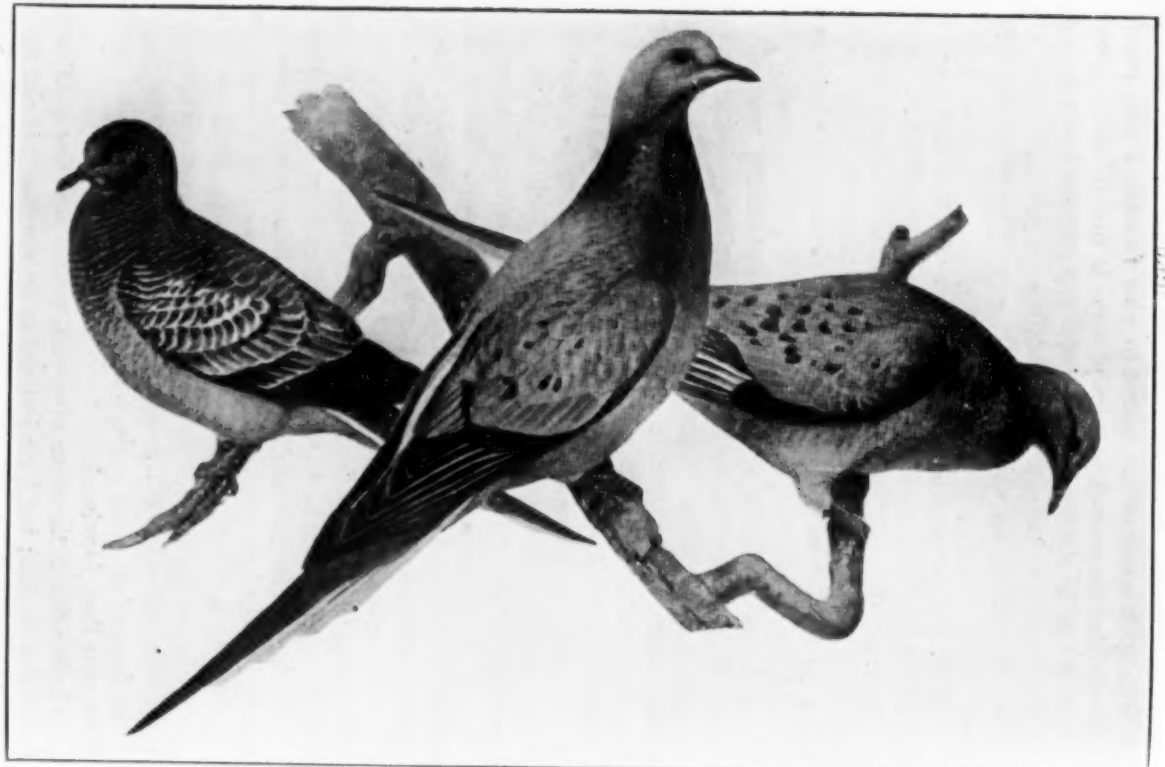

FIG. 3. A GROUP OF WILD PIGEONS FROM EATON'S "BIRDS OF NEW YORK." LOUIS AGASSIZ FUERTES PAINTED THIS FINE PICTURE, WHICH CONSISTS OF AN ADULT PAIR AND A BIRD OF THE YEAR

what, and the tree in the middle distance is absent. But in the background we find the same scene as in Goodrich, apart from a few trivial changes. This cut is on page 28 of Volume I. of Nuttall—the Land Birds—the account of the Wild Pigeon being on pages 628 to 635 inclusive. As this volume is dated 1832, or twenty-nine years before the Goodrich volume was published, it is evident that a still earlier cut of the kind existed, which was drawn upon by both the authors, or, what is more likely, Goodrich's engravers or his artist made up a wild pigeon scene for the work, and copied Nuttall's figure in the foreground, simply turning it to the right and slightly reducing it. Such procedures were by no means uncommon in those times—a fact one soon appreciates after studying the various published pictures of the Wild Pigeon. Nuttall acknowledges who engraved for him in his Preface to Volume I., on page vi., where he says: "The wood engravings, not sufficiently numerous in consequence of their cost, have been executed by Mr. Bowen, of Boston, and Mr. Hall, in the employ of Messrs. Carter and Andrews, of Lancaster." Cambridge was the place of publication.

To illustrate the word "pigeon" or "passenger-pigeon," we sometimes find our extinct wild one selected for the purpose, and a good example of this is seen in the case of the "Century Dictionary," where Thompson-Seton gives us a figure that is far above the average of such cuts in points of excellence.

Perhaps Wilson's rather quaint but attractive figure of the bird has been more extensively used as the basis for smaller cuts than that of any other artist. For example, Tenney used it in his text-book on zoology, and Coues, borrowing it from him, reproduced it on page 711 of Volume II. of the fifth edition of his "Key to North American Birds." To some extent, this cut was altered; for, as we know, Wilson represented his wild pigeon as standing on the top of a sawn-off stump of a tree, while the cuts in Tenney's and in Coues have the bird standing on the ground. In doing this, no change was made in the posing of the feet.

As we know, T. M. Brewer published an edition of Wilson's "American Ornithology" in 1852; and of all the colored plates known to me, the ones illustrating this work are the most unsatisfactory and incorrectly colored. They were reduced from the plates in Wilson's folio edition to a three-half, six-half size; and in my personal copy of this work, the plate carrying the wild pigeon is so inserted as to cause the bird to be up-side down. It has been tinted a curious shade of purple, with pale purple outer tail feathers, and with a bright pink breast. In this respect Audubon fared much better; for, with the exception of a few indifferent cuts based on his magnificent plate of a pair of Wild Pigeons, the latter has been reproduced in color in several works, a

FIG. 3. A GROUP OF WILD PIGEONS FROM LATONS "BIRDS OF NEW YORK." LOUIS AGASSIZ FUERTES PAINTED THIS FINE PICTURE, WHICH CONSISTS OF AN ADULT PAIR AND A BROOD OF THE YEAR

FIG. 4. REPRODUCTION OF AUDUBON'S PLATE OF THE WILD PIDGEON

strikingly beautiful example of which may be seen in that most valuable and interesting book on the bird by W. B Mershon, given us in 1907 by the Outing Publishing Company. This volume has two reproductions of photographs of mounted specimens of the Passenger Pigeon that are above the average in the point of excellence; and, finally, it has a beautiful plate of Dr. C. O. Whitman's photograph from life of an immature bird of this species. (Fig. 2).

An admirable plate in color of a pair of Passenger Pigeons by Fuertes occurs as the frontispiece to the work just mentioned, the same having been used by various other authors. This painting was done in 1904, since which time, for all I know to the contrary, this most industrious avian artist may have given us other colored plates of this species,—at least I find a very beautiful one, and I may say a very faithful one, in the first volume of Eaton's magnificent work "The Birds of New York," where it is shown on Plate 42, upper figure, the group consisting of a pair of adults and a young bird. (Fig. 3). In my opinion, this is one of the most accurate, and decidedly the most pleasing of all the colored figures of the Wild Pigeon that have appeared up to date. It leads Audubon's plate for the reason that it is such a restful one to study, while in the case of Audubon's, the error he committed in so many of his representations of birds is there repeated—that is to say, that in technical ornithological works the portraits of birds should never be shown in unusual poses or performing some action. (Fig 4). In this criticism I found myself in agreement with the late, very distinguished British Ornithologist, Alfred Newton, who, many years ago, wrote me to that effect.

Eaton's "Birds of New York" bears date of 1910—that is, three years after Mershon, and two years before the splendid volume of Forbush appeared on "A History of the Game Birds, Wild Fowl, and Shore Birds of Massachusetts and Adjacent States"—a work too well known to ornithologists to require description here. In it we find three plates devoted to the Passenger Pigeon, one being of a beautifully mounted specimen, while the remaining two are of exceptional value, in as much as they are reproductions of photographs of living specimens of the bird itself. In so far as my knowledge carries, these are the only pictures of the kind extant. I have already referred to one of them as being an illustration in Mershon's work "The Passenger Pigeon," that is, the one reproduced from C. O. Whitman's photograph; the other, here to be noticed, is the reproduction of the last of all the Passenger Pigeons that ever lived: It is the Enno Meyer photograph, taken of the bird when it lived in the Zoological Garden of Cincinnati. (Fig. 5). It is quite unnecessary to comment on the value of this picture or its uniqueness, as it represents one of those things that can never be repeated.

FIG. 5. THE PASSENGER PIGEON IN LIFE. PHOTOGRAPHED BY ENNO MEYER OF THE BIRD THAT LIVED IN THE ZOOLOGICAL GARDENS OF CINCINNATI, OHIO

FIG. 6. SAME SPECIMEN AS SHOWN IN FIGURE 5. MOUNTED AFTER ITS DEATH BY THE LATE MR. NELSON R. WOOD, AND NOW IN THE EXHIBITION SERIES OF THE UNITED STATES NATIONAL MUSEUM

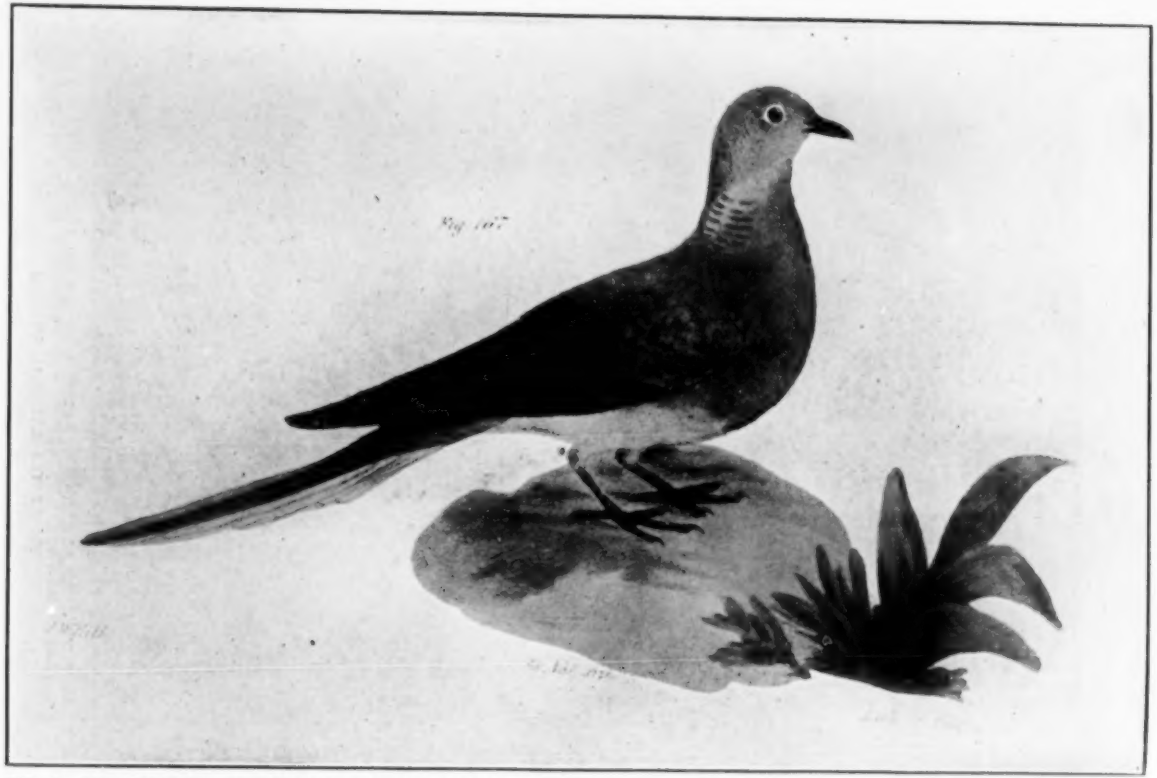

FIG. 7. THE COLORED FIGURE OF THE WILD PIGEON AS SHOWN IN THE WORK OF JAMES DE KAY ("NEW YORK FAUNA.")

This bird, after its death, was preserved by Mr. Wm. Palmer at my residence, and I have published an account of its gross anatomy. There are three other negatives of it posed by me at the photographic galleries of the United States National Museum, and they present the three different views of the specimen, namely the ventral, the dorsal and the lateral ones. After this specimen was mounted, I was permitted to make a photograph of it, and this latter has been published in *The Conservationist*, of Albany, N. Y., and in *American Forestry*, of Washington. (Fig. 6).

If there be a figure of the Wild Pigeon in "The Game Birds of the United States" by the late Dr. D. G. Elliott, I do not recall it, for a copy of that work has not been in my hands for many years. This is not the case, however, with respect to the "Zoology of New York," or the "New York Fauna," by James E. de Kay. Part II. of this well-known and much criticized work is devoted to the Birds, and on page 196 there is an half-page account of "*Ectopistes migratoria*." The figure of the bird in color, five-eights natural size, is a male, engraved by J. W. Hill and lithographed by Endicott of New York. It is Plate 74, being a rather pleasing, not to say fairly correct representation of the species. (Fig. 7).

Coues, in his Biographical Appendix, gives the date of publication of this work as 1884, and says that the birds "are figured in colored lithographs, each plate containing two or three figures. The plates are all recognizable illustrations, but not of the highest order of artistic merit, the drawing being especially defective." (p. 633.)

There has been published at least one plate on which is given no fewer than fourteen Passenger Pigeons, representing both sexes and young in apparently typical plumages. This is Plate XXIX, opposite page 32 of "Studer's Popular Ornithology—The Birds of North America,"—a work illustrated throughout by Dr. Theodore Jasper, and edited and published under copyright in 1881 by Jacob H. Studer and Co., of New York and Columbus, Ohio. (Fig 8). Neither the text nor the plates of this folio volume seem to have met with favor in the eyes of ornithologists anywhere; but of all this interesting history nothing will be recorded here.

To appreciate Alexander Wilson's figure of the Wild Pigeon, one should see it in Volume V. of his folio set, which was published in Philadelphia in 1812. We find it to be Figure I. on Plate 44, opposite page 102, where it is designated as *Columbia migratoria*, and represented to be of natural size. On the same plate we find Figure II., the Blue Mountain Warbler, and Figure III., the Hemlock Warbler (Fig 9). There is a peculiar quaintness and charm about Wilson's figures of birds that attaches but to few others. I must believe that their pathetic history has something to do with all this, for we know that Wilson drew all his own figures of birds, while they were engraved

FIG. 8. DOCTOR JASPER'S GROUP OF THE FOURTEEN WILD PIGEONS, FROM STUDER'S "THE BIRDS OF NORTH AMERICA." (PL. XXIX.)

by I. G. Warnicke, of the firm of Messrs. Lawson, Murray, and Warnicke, his printers being Messrs. R. and W. Carr, a Philadelphia firm of note in those days.

There is but one criticism I would make of the Wild Pigeon as portrayed by Wilson; it is that the sawn-off stump upon which it stands is altogether too small. As the bird had a length between 16 and 17 inches, we can readily calculate what the diameter of that stump must have been. Surely the tree could not have been of a size sufficiently large to demand *sawing across* to fell it! This discrepancy has doubtless been observed by others—hence the placing of Wilson's Wild Pigeon on the ground in some of our modern text-books in zoology.

On page 10 of his preface, Wilson gives us a paragraph, the sentiment of which is quite as true to-day as in his time; he says: "Let but the generous hand of patriotism be stretched forth to assist and cherish the rising arts and literature of our country, and both will most assuredly, and that at no remote period, shoot forth, increase, and flourish with a vigor, a splendor, and usefulness inferior to no other on Earth."

In skimming through that most useful piece of work, the Biographical Appendix of Dr. Elliott Coues, we meet with various other works, of a minor sort or otherwise, in which cuts of the Wild Pigeon occur, or may occur, as those of E. A. Samuels, W. L. Bailey, W. P. Turnbull, and others. In the important ornithological works of Baird, Brewer, and Ridgway, only the heads of the birds described are figured; while in Mr. Ridgway's well-known "Manual" we find but an excellent character drawing, giving in outline simply the head, wing, tail, and foot of the Passenger Pigeon.

There is a quaint figure of the bird under consideration in the early work of P. Kalm, published in 1772, and entitled "Travels Into North America," with a very lengthy sub-title. The plate of the Wild Pigeon is opposite page 74; while in 1785, or thirteen years after Kalm published, there appeared the well-known classic of T. Pennant, entitled "Arctic Zoology." Here a very crude engraving of the Passenger Pigeon is given on the same plate (which is No. XIV.) with the Caroline Dove. (Fig. 10). This I examined in a copy of the work formerly in the personal library of the late Dr. Edgar A. Mearns, which is now in the library of the United States National Museum. Who engraved this plate is a fact still unknown to me, while the work was printed by Henry Hughs, of London. Volume II. is devoted to the Birds, which are grouped in Class II., and it is on page 322 that we find treated Order IV., the *Columbine*, under which a brief account of the Wild Pigeon is given.

Ornithologists are familiar with the remarkable history that attaches to the great folio work on Pigeons, of which C. J. Temminck is

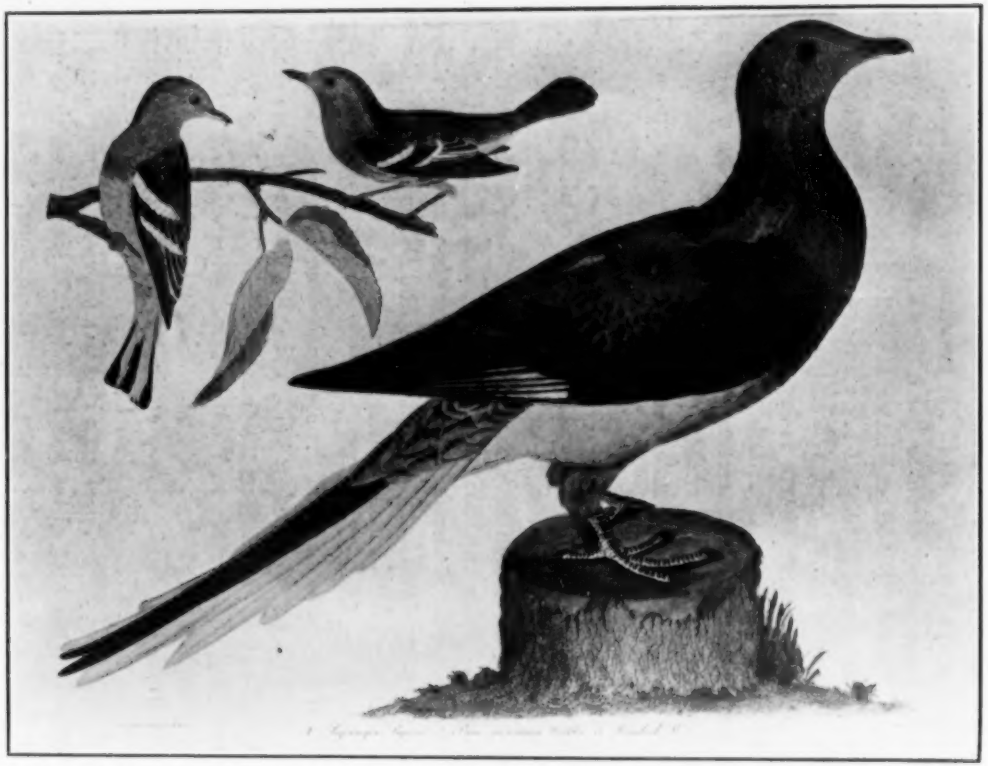

FIG. 9. THE PASSENGER PIGEON AS GIVEN US BY ALEXANDER WILSON. FIGURE 1 ON PLATE XLIV. OF HIS FOLIO SET

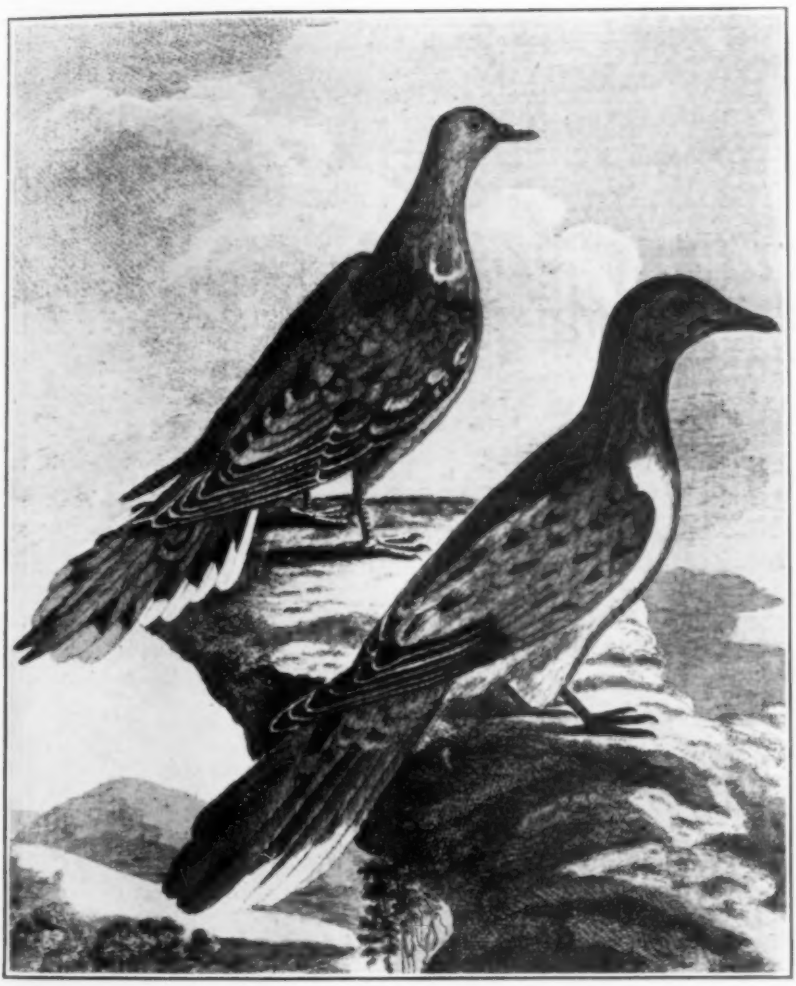

FIG. 10. PLATE FROM PENNANT'S "ARCTIC ZOOLOGY." CAROLINA DOVE IN THE FOREGROUND AND WILD PIGEON BEYOND. (PL. XIV.)

FIG. 11. MADAME KNIP'S PAINTING OF THE PASSENGER PIGEON, WHICH WAS PUBLISHED IN HER PIRATED EDITION AS WELL AS IN TEMMINCK'S. THE LATTER BEING THE AUTHOR OF THE TEXT OF THE WORK

the author, and which was published in 1808 to 1811; and how it was pirated, as a whole, by Madame Knip, née Pauline de Courcelles, at the time it was issued. Madame Knip was the artist who painted the life-size colored figures of the large number of pigeons figured in this work, while they were engraved by Cesar Macret, of Paris. Each species is given a plate to itself, and that of the Wild Pigeon is No. 48, which is said to be a male bird. As an artistic picture, it is excellent; but as a correct figure of the species it purports to represent, it is a failure. The model was evidently a skin, and this may account for the small head and bill, but not for the short tail that Madame Knip has endowed it with. (Fig. 11).

In 1857 to 1858, Charles Lucien Bonaparte published his magnificent folio, entitled "Iconographie des Pigeons;" but as the plates only gave such species as were not figured by Madame Knip, the Passenger Pigeon does not appear in it.

We next have to consider the work of Heinrich Gottlieb Ludwig Reichenbach, who was born at Leipzig on the 8th of January, 1793, and who died March 17, 1879, which made him eighty-six years of age at the time of his death. He was the author of a work on the Columbidæ, entitled "The Complete Account of the Natural History of the Pigeons and Pigeon-like Birds," a copy of which I have examined. It appeared in Dresden, in the German language, as a folio volume apart from the text, and illustrated with colored plates. With respect to the text of this work, I am indebted to Dr. Richmond for the opportunity to examine it. It was probably published about 1861, being unbound, and of a much smaller size than the plates. The account of the Wild Pigeon on pp. 81-85 is chiefly from Audubon and others. Plate 154 of the bound plates is devoted to *Ectopistes migratorius* (Fig 12), of which there are three figures in color resting on the limb of a tree; they are numbered 1374, 1378, and 1379, and all three are but indifferent representations of the species he aimed to delineate.

Reichenbach evidently got his middle figure of the Wild Pigeon from John Prideaux Selby's work, entitled "The Natural History of Pigeons," which appeared in Edinburgh in 1835, being one of the demi-octavos of *The Naturalists' Library*, edited by Sir William Jardine. It is the volume devoted to the Pigeons, and is illustrated by 32 colored plates of those birds together with numerous woodcuts. An account of the Passenger Pigeon, or, as Selby called it, the "Passenger Turtle," is given in Volume V. on pages 177 to 188 inclusive, the colored figure of the bird being Plate 19, opposite page 176. There is no question but that Reichenbach reproduced this figure in his plate; changed the limb and scenery, and then added another figure of the pigeon on either side of it, which he may possibly have obtained from still other sources. In doing this, the Selby figure was somewhat reduced

FIG. 12. A GROUP OF PASSENGER PIGEONS FROM THE ORNITHOLOGICAL WORK OF REICHENBACH'S

in size. E. Lear drew Selby's figures of the pigeons, and they were engraved by Lizars.

We may now enquire as to who published the first figure or plate representing the Wild Pigeon; and, in so far as I have been able to discover, it would seem to have been Mark Catesby, whose elephant folio work appeared in 1771; it is entitled "The Natural History of Carolina, Florida, and Bahama Islands, with a lengthy sub-title. The colored plate of the Wild Pigeon occurs in Volume I., and is Plate 23, its caption being "The Pigeon of Passage" (Palumbus Migratorius). (Fig. 13).

The bird is quite recognizable, although figured in the quaint style so characteristic of the ornithological artists of those times. The accessories consist of the leaves and acorns of the Red Oak, the bird standing on the upper surface of one of the separated leaves, the indications being that the leaf is on the ground and not floating in mid-air.

There is an elaborate Preface to this work (pp. V.-XII.), on page XI of which Catesby tells us that "As I was not bred a Painter, I hope some faults in Perspective, and other Niceties may be more readily excused, for I humbly conceive Plants, and other Things done in a Flat, tho' exact manner may serve the Purpose of Natural History, better in some Measure than in a bold and Painter like way. In designing the Plants, I always did them while fresh and just gathered: And the Animals, particularly the Birds, I painted them while alive (except a few) and gave them their Gestures peculiar to every kind of Bird, and where it would admit of, I have adapted the Bird to those Plants on which they fed, or have any Relation to."

Catesby had considerable trouble, on account of the expense, in securing an engraver; but as he adds in his Preface, "At length by the Kind Advice and Instructions of that inimitable Painter Mr. Joseph Goupy, I undertook and was initiated in the way of Etching them myself, which, though I may not have done in a Graver-like manner, choosing rather to omit their method of cross-Hatching, and to follow the humour of the Feathers, which is more laborious, and I hope has proved more to the purpose."

Next follows a long discussion of the colors used in this work, and other matters of interest.

This ancient classic is still consulted from time to time, and we turn to it for many reasons in a reverential way; and by no means the least one of the reasons is, that nearly a century and a half ago, its author published for us a plate of the Passenger Pigeon, little dreaming as he did so that this splendid species, then existing in unnumbered millions in this country, would so soon be utterly exterminated by those living in the regions where it occurred.

It would appear that Count de Buffon never published a figure or

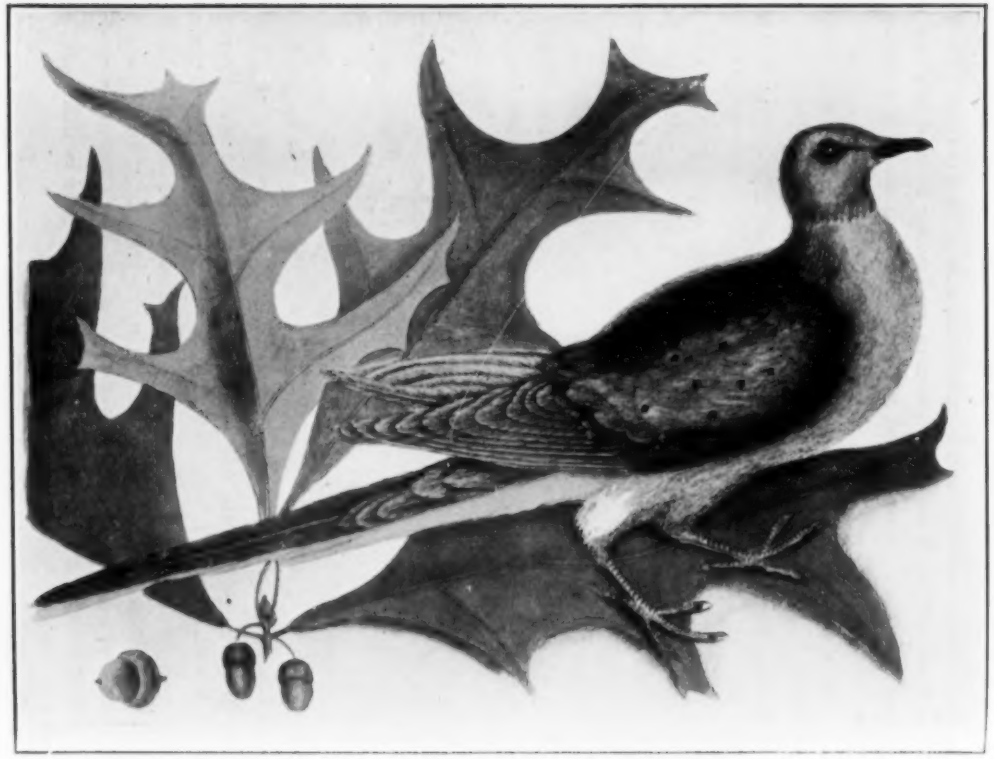

FIG. 11. PROBABLY THE FIRST PLATE OF THE WILD PIGEON PUBLISHED. A PHOTOGRAPHIC COPY OF THE PIGEON IN MARK CATESBY'S ORNITHOLOGY (1771)

FIG. 14. HAYASHI'S FIGURE OF THE MALE WILD PIGEON IN THE WORK OF DR. C. O. WHITMAN

FIG. 13. PROBABLY THE FIRST PLATE OF THE WILD PIGEON PUBLISHED — A PHOTOGRAPHIC COPY OF THE PIGEON IN MARK CATESBY'S ORNITHOLOGY (1731)

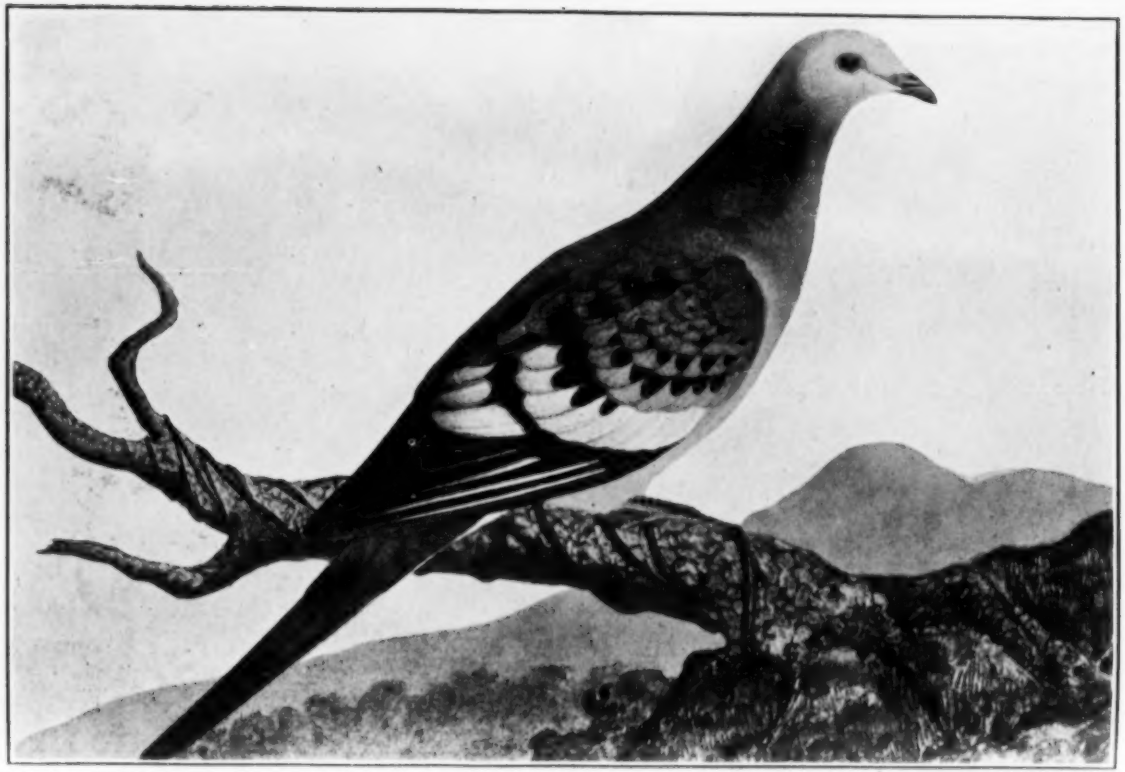

FIG. 15. BY THE SAME ARTIST WHO PAINTED THE PIGEON HERE SHOWN IN FIGURE 14. THIS IS THE FEMALE, AND IS PLATE 29 OF WHITMAN'S WORK ON THE PIGEONS

plate of the Wild Pigeon; but we find one in Daubenton that appeared in about the year 1780. This latter is a small quarto with colored plates, but no text. It was intended to illustrate, or rather be a complimentary work to Buffon, illustrating what the latter had published on birds. Daubenton gives a colored plate, No. 176, of an immature Passenger Pigeon, which he designates as the "Tourterelle du Canada" which is recognizable, but hardly anything more; it is about two-thirds the size of life.

Thomas E. Eyton published, in 1836, a small octavo in London, which he entitled "A History of the Rarer British Birds." On page 30 there is a small woodcut of the Passenger Pigeon which is fairly good, and he says of the species that "Our authority for introducing it into this work, as a member of the British Fauna, rests upon a specimen mentioned by Dr. Fleming in his 'History of British Animals,' shot at Westhall, in the parish of Monymeal, Fifeshire, on the 31st of December, 1825. The feathers were quite fresh and entire, like those of a wild bird. The specimen in question was presented to Dr. Fleming by the Rev. A. Esplin, schoolmaster at Monymeal." This specimen was evidently a "straggler" and very different from introduced birds, such as the lot that Audubon is responsible for turning loose in England in 1830—an exploit described in Smart's "Birds of the British List."

I have stated that it was perhaps Catesby who published the first plate of our Wild Pigeon; and it may now be asked: who holds the honor of having published the last plate of the bird? This is an event of only about a year ago, when the posthumous works of Charles Otis Whitman appeared. This great treatise, entitled "Inheritance, Fertility, and the Dominance of Sex and Color in Hybrids of Wild Species of Pigeons," is edited by Mr. Oscar Riddle, and published by the Carnegie Institution of Washington in four handsome quarto volumes. Its carefully executed colored plates were engraved by the Hoen Company, of Baltimore, and two plates of the Passenger Pigeon occur in the second volume. They are reproductions of the work of the well-known Japanese artist, Hayashi. Plate 28 (Fig. 14) represents an adult male bird (x. 06), and has not a little to recommend it. It may be suggested, however, that the limb upon which the specimen is represented as standing, is too vertical for the pose the artist has given the bird.

The female, to which Plate 29 is devoted, (Fig. 15) is better, and to me, a far more pleasing figure. It is of an adult individual and beautifully tinted (x o .05). Mr. Hayashi also painted the picture of which this plate is a copy—indeed, I believe he is responsible for all the colored plates that illustrate this superb work—a veritable monument to the department of scientific ornithology of which it treats.

FIG. 15. BY THE SAME ARTIST WHO PAINTED THE PIGEON HERE SHOWN IN FIGURE 14. THIS IS THE FEMALE, AND IS PLATE 29 OF WHITMAN'S WORK ON THE PIGEONS

# THE PROGRESS OF SCIENCE

## PROFESSOR EINSTEIN'S VISIT TO THE UNITED STATES

Plans have been under consideration for lectures by Professor Einstein in the United States, but his arrival at the beginning of April on a mission to promote the Zionist movement was a surprise. He is accompanied by Professor Chaim Weizmann, director of the chemical research laboratories of the British Admiralty during the war, now head of the World Zionist Organization, and two other leaders in the movement. Professor Einstein is reported to be especially interested in the establishment of a University of Jerusalem and to be ready to take part in its work, but it is not likely that he will leave Berlin permanently. Professor Bergson has denied the report that he would leave Paris to become professor at Jerusalem.

Arrangements were promptly made for scientific lectures by Professor Einstein at several universities, the first being appropriately given at Columbia University, which awarded to him last year the Barnard Medal on the recommendation of the National Academy of Sciences. Four lectures have been given at the College of the City of New York and a series of five lectures is announced to be given at Princeton University from May 9 to 13. Scientific men are invited to attend the Princeton lectures; those wishing to do so should write to Professor H. A. Thompson.

It is satisfactory that there should be such widespread popular interest in Professor Einstein and his work. In the article on the History of Mathematics by Professor Ernest Brown in the present issue of the MONTHLY and in the article on the History of Physics by the late Professor Andrews Henry Bumstead in the last issue will be found statements of Professor Einstein's contributions in their historic continuity. An article on the Theory of Relativity by Professor E. B. Wilson was printed in the issue of the MONTHLY for March, 1920. In the issue of *Nature* for February 17 last will be found a series of articles on all aspects of the theory of relativity. Professor Einstein himself contributes an article on the development of his theory in which he writes:

> The development of the special theory of relativity consists of two main steps, namely the adaptation of the space-time "metrics" to Maxwell's electro-dynamics, and an adaptation of the rest of physics to that altered space-time "metrics." The first of these processes yields the relativity of simultaneity, the influence of motion on measuring-rods and clocks, a modification of kinematics, and in particular a new theorem of addition of velocities. The second process supplies us with a modification of Newton's law of motion for large velocities, together with information of fundamental importance on the nature of inertial mass.
>
> It was found that inertia is not a fundamental property of matter, nor, indeed, an irreducible magnitude, but a property of energy. If an amount of energy E be given to a body, the inertial mass of the body increases by an amount $E/c^2$, where $c$ is the velocity of light *in vacuo*. On the other hand, a body of mass $m$ is to be regarded as a store of energy of magnitude $mc^2$.
>
> Furthermore, it was soon found impossible to link up the science of gravitation with the special theory of relativity in a natural manner. In this connection I was struck by the fact that the force of gravitation possesses a fundamental property, which distinguishes it from electro-magnetic forces. All bodies fall in a gravitational field with the same acceleration, or—what is only another formulation of the same fact—the gravitational and inertial masses of a body are numerically equal to each other. This numerical equality suggests identity in character. Can gravitation and inertia be identical? This question leads directly to the General Theory of Relativity. Is it not pos-

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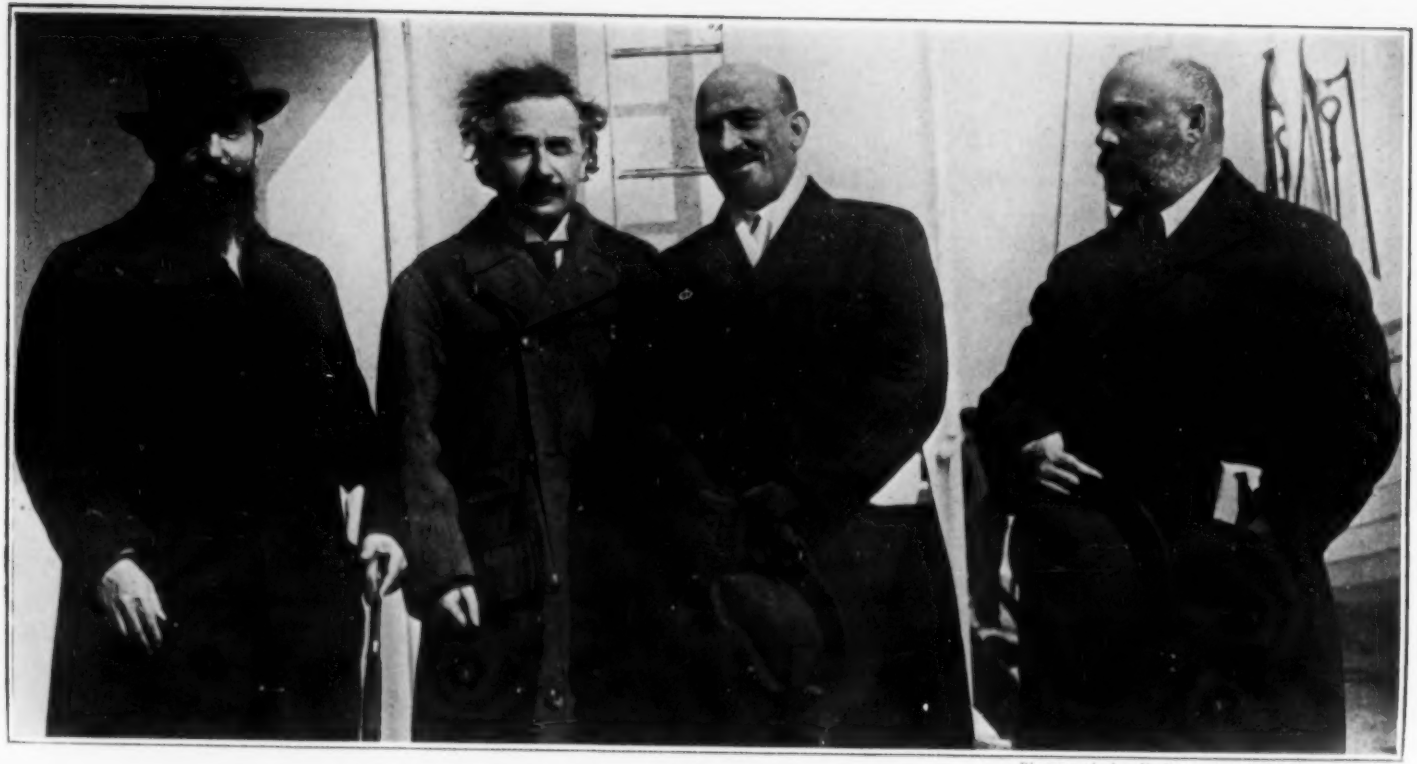
PROFESSOR ALBERT EINSTEIN



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LEADERS OF THE WORLD ZIONIST ORGANIZATION ON THEIR ARRIVAL AT NEW YORK

PRINCIPAL MOSSINSON — PROFESSOR EINSTEIN — DR. WEIZMANN — DR. USSISHKIN

sible for me to regard the earth as free from rotation, if I conceive of the centrifugal force, which acts on all bodies at rest relatively to the earth, as being a "real" field of gravitation, or part of such a field? If this idea can be carried out, then we shall have proved in very truth the identity of gravitation and inertia. For the same property which is regarded as *inertia* from the point of view of a system not taking part in the rotation can be interpreted as *gravitation* when considered with respect to a system that shares the rotation. According to Newton, this interpretation is impossible, because by Newton's law the centrifugal field can not be regarded as being produced by matter, and because in Newton's theory there is no place for a "real" field of the "Koriolis-field" type. But perhaps Newton's law of field could be replaced by another that fits in with the field which holds with respect to a "rotating" system of coordinates? My conviction of the identity of inertial and gravitational mass aroused within me the feeling of absolute confidence in the correctness of this interpretation. In this connection I gained encouragement from the following idea. We are familiar with the "apparent" fields which are valid relatively to systems of coordinates possessing arbitrary motion with respect to an inertial system. With the aid of these special fields we should be able to study the law which is satisfied in general by gravitational fields.

## A NEWS SERVICE FOR SCIENCE

Science Service is the name of an agency newly established in Washington for the diffusion of knowledge. It is generously supported by Mr. E. W. Scripps and will be a corporation conducted without profit, all receipts being used for the work and its extension.

The Service will pay adequately for notes and articles that are scientifically correct and of popular interest and will dispose of them through the existing news syndicates and in other ways that will provide the largest possible circulation. It plans also to take up motion pictures and all other methods useful for the popularization of science.

The first board of trustees of Science Service is composed as follows:

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William Allen White, editor, *The Emporia Gazette*, Emporia, Kansas.

Dr. W. E. Ritter is president of the board, Mr. R. P. Scripps, treasurer, and Dr. Vernon Kellogg, vice-president and chairman of the executive committee. This committee is

DR. ERNEST FOX NICHOLS
PRESIDENT OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

composed of five members, one selected from each group of trustees from the different organizations represented on the board. The present members of the committee are the president and vice-president of the board, Dr. J. McKeen Cattell and Dr. J. C. Merriam. A member from the journalistic group is yet to be selected.

As editor the board of trustees has selected Edwin E. Slosson, Ph.D., who for twelve years was professor of chemistry in the University of Wyoming and for seventeen years literary editor of *The Independent*, New York. He has been associate in the Columbia School of Journalism since its foundation and is the author of "Creative Chemistry," "Easy Lessons in Einstein," and other scientific and literary publications.

As manager of the new enterprise the board has selected Howard Wheeler, formerly editor of the San Francisco *Daily News*, Pacific coast manager of the Newspaper Enterprise Association, managing editor of *Harpers Weekly*, and for five years editor of *Everybody's Magazine*.

The headquarters of Science Service have been provisionally established in the building of the National Research Council, at 1701 Massachusetts Avenue, Washington, D. C.

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## THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND PRESIDENT NICHOLS

The election of Dr. Ernest Fox Nichols, as president of the Massachusetts Institute of Technology, was announced by the corporation on March 30. Dr. Nichols succeeds the late Dr. Richard C. Maclaurin, also a distinguished physicist, under whose administration the institute moved to its new buildings and made notable progress in its educational work.

For the last twelve months Dr. Nichols has been director of physical research at the Nela Park Laboratory of the National Electric Lamp Association, Cleveland. He was born in 1869 at Leavenworth, Kansas, graduated from the Kansas Agricultural College and received from Cornell University the degree of doctor of science in 1897. In 1892 Dr. Nichols was appointed to the chair of physics and astronomy at Colgate University, where he remained for six years. More than two years of this time, however, was spent on leave of absence during which he studied at the University of Berlin. There he discovered the metallic reflection of quartz and its anomalous dispersion in the infra-red spectrum, which led to a new method of spectrum analysis by which the spectrum was extended to six times the previous limits. Rubens, Wood and von Bayer were thus enabled to make a further extension, detecting heat waves 1/64 inch in length.

In 1898 Dr. Nichols was called to the professorship of physics in Dartmouth College, where he made the first measurements of the heat received from several of the brighter stars and planets, by using a radiometer of his own invention, and with Dr. Hull, in 1901, discovered the pressure of a beam of light which had been predicted by Maxwell. Simultaneously the Russian physicist, Lebedev, was able to detect this pressure, but unable to measure it.

After five years at Dartmouth, Dr. Nichols was called to the chair of experimental physics in Columbia University. The year 1904-05 Dr. Nichols spent at Cambridge, England, and lectured at the Royal Institution in London and the Cavendish Laboratory of Cambridge University. He remained at Columbia until 1909, when he was called to the presidency of Dartmouth, resigning in 1916 to become professor of physics at Yale University. This latter position he held until 1920, but during the war he was associated with the Bureau of Ordnance of the Navy Department.

## SCIENTIFIC ITEMS

We record with regret the death of John Burroughs, the distinguished naturalist; of Henry P. Cushing, professor of geology in Western Reserve University, and of Louis Compton Miall, the English biologist.

The annual meeting of the American Philosophical Society at Philadelphia and of the National Academy of Sciences at Washington were held toward the end of April. The evening lecture before the American Philosophical Society was given by Professor James H. Breasted, of the University of Chicago, whose series of lectures on "The Origins of Civilization" were recently printed in this journal. Prince Albert of Monaco gave the evening address before the National Academy of Sciences, and its Alexander Agassiz gold medal was conferred on him in recognition of his promotion of oceanographic research.

The Albert medal of the Royal Society of Arts has been presented to Professor Albert Michelson, for his discovery of a natural constant which has provided a basis for a standard of length. The award was made last year, but the actual presentation was deferred until Professor Michelson could go to England to receive it.

Dr. William Crocker, associate professor of botany in the University of Chicago, has been appointed director of the newly founded Thompson Institute for Plant Research at Yonkers, New York. He will enter on his work next autumn. The board of trustees of the new foundation will consist of three business men and three scientific men. Professor John M. Coulter, head of the department of botany at the University of Chicago, and Raymond F. Bacon, of the Mellon Institute of Pittsburgh, will be two of the scientific men, and these two will select the third.

*From Punch.*

NEWTON AND THE RELIC HUNTER: AN APPLE NOT A STONE